THE METHOD OF SOCIOLOGY

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PREFACE

Every vital science changes continually. Not only are new facts being discovered and new theories constructed all the time, but the ways of searching for facts and the methods of constructing theories undergo a ceaseless evolution. This evolution is always more or less conscious and intentional, which means that it is accompanied by methodological reflection.

Sometimes changes are so rapid and profound as to bring a crisis. The essential principles of investigation and systematization which were universally recognized during a certain period cease to be regarded as valid and other principles must be elaborated. At such times, methodological reflection assumes a leading rôle in formulating new ideals of scientific achievement. It did this for the entire knowledge of antiquity during the period extending from Protagoras to Aristotle. It performed a similar function for all natural science from the middle of the sixteenth to the middle of the seventeenth century. Recently, at the beginning of this century, its influence assisted powerfully in bringing about the radical reconstruction which the physical and chemical sciences have undergone.

Now, sociology is passing through a crisis as deep as any science ever passed through. It was established as a synthetic science of "society" or "civilization," using the results of several other sciences to draw such comprehensive generalizations as none of those sciences could or cared to draw for itself. It is changing into an

analytic science investigating directly and independently particular empirical data, formulating its own results in a vast monographic literature, and not only avoiding hasty conclusions, but often mistrusting generalization more than other sciences do, and more than is good for any science. In this crisis it needs all the light which methodological studies and discussions can throw on its present and future.

Many students absorbed in special research are inclined to undervalue the importance of methodology or even to deny it altogether. Science, they claim, is only advanced by positive investigation. What is the use of discussing how things ought to be done? Go and do them. It is the artist and not the philosopher of art who creates new aesthetic values, the moral leader and not the student of ethics who introduces new standards of conduct, the statesman and not the political scientist who leads political life, the business man and not the economist who makes wealth.

But there is an obvious fallacy in this argument. Art, morality, politics, business are not theoretic pursuits. Theoretic study is not a part of their function: it is at best an instrument which they use in defining their practical aims. Whereas scientific activity is theoretic activity, and methodological reflection is inseparably associated with it. It plays the same part in scientific progress as the conscious expression of new aesthetic ideals in the evolution of art, as the formulation of new moral ideals in the progress of practical morality, as critical and reflective consideration of new political possibilities in statesmanship, as outlining new enterprises and new ways of management in business. A science directed by methodology in contrast to a science proceeding by undirected monographic contributions rep-

resents a stage of intellectual development parallel to modern planful technology in contrast to the trial-and-error techniques of the past.

Among those scientists who realize that methodological reflection is useful and even necessary, there are some who demand that it be always connected with instances showing how certain methods are actually applied in scientific research. The famous collective book, *Methods in the Social Sciences*, edited by Stuart F. Rice (Chicago, 1931), originated in such demands. There is no doubt as to the great value of this kind of critical and constructive analysis of the ways in which scientific problems are really defined and solved in the original work of individual scientists. But there is one function which this type of methodology fails to fulfil. It does not attempt to formulate general scientific ideals for the future. However successful may be a particular scientific achievement, it always and inevitably falls short of the scientist's highest standards of perfection, and it is essential to know those standards as the goal toward which he is striving. However important and original may be an individual's scientific contribution, the use which will be made of it for the advance of science depends on the common or prevalent direction in which the work of other scientists in the same and neighboring fields is moving. In addition, therefore, to critical reflection about methodological innovations as exemplified in concrete studies, sociology (like every other science) needs fundamental discussion concerning the general possibilities and conditions of its future development.

The present book embodies the result of long and strenuous efforts to harmonize ideals with reality, to reconcile the standards of highest scientific perfec-

tion, derived partly from philosophy, partly from the methodologies of physical and biological sciences, with the need for preserving intact those characteristics which concrete social facts possess in our experience. It has been worked out in a continual conflict between the interests of exact analysis and strictly rational systematization on the one hand, and the interests of unprejudiced observation and empirical research with their inexhaustible variety of materials, on the other.

This conflict has driven the author to exclude from the field of sociology all but one specific category of data, in contrast to the more comprehensive ambitions of most sociologists, and while rejecting the "formalistic" views of Simmel and his followers, to conceive of it as a special science, limited to those facts it can successfully cope with. Under the same conflicting influences the author has been forced to emphasize, in opposition to materialistic schematism, the primary and essential meaningfulness of social reality, to accept human values and activities as facts, just as human agents themselves accept them, but to study them objectively and with the application of the same formal principles as the physicist and the biologist apply to material nature. The same conflict has made the author aware that at the present stage of scientific analysis attempts to rationalize social reality quantitatively often sacrifice the substances of valuable knowledge and true discovery for the shadow of mathematical formulae devoid of significant content; but at the same time it made him strive to maintain in his qualitative studies the highest standard of logical exactness compatible with the nature of social data.

The ideal thus reached, as expressed in the present book, will probably be judged by some insufficiently

strict and objective, leaving too much free play to the "subjective" experience and interpretation of the sociologist; while others may think it too scholastic, impossible to apply in actually dealing with the mass of concrete facts. I should answer the first objection by pointing out that no way has been found as yet to eliminate from the study of human facts the individual understanding of the student, without eliminating that which makes those facts real to all men. The second objection ought to be met by showing the results of research carried on in accordance with the principles here laid down. And, indeed, the present book was originally written as a mere introductory part of a large work summarizing the results of the author's sociological studies. It is now being published as a separate volume for several reasons, not the least of which is the consciousness of the author that in his positive work he is far from living up to his own standards. Let, therefore, this abstract expression of a scientific ideal stand by itself, to be followed by a series of more or less imperfect, partial attempts at realization.

My obligations as a sociologist are too numerous to be recorded. But there are two men to whom above all others I wish to express my gratitude at the commencement of the publication of my sociological results. One is William I. Thomas, a long and intimate collaboration with whom was the best possible introduction a philosopher could have had into sociological reality. The other is Robert M. MacIver, whose constructive criticism helped me most in the final formulation of my theories.

F. Z.

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THE METHOD OF SOCIOLOGY

CHAPTER I

The Selection and Determination of Scientific Data

1. *Practical Standards of Selection in Sociology*

Every one of the sciences dealing with empirical reality makes in the course of its development a continuous selection of those objects and facts which it means to study as belonging to its particular field of research. This selection is in part the result of previous research, which has left certain hypotheses to test and certain problems to solve; it is chiefly due, however, to methodological reflection as to the possibilities and limits of future discovery and systematization. In "practical" sciences, like engineering, medicine and jurisprudence, which aim at a direct application of their results to the achievement of technical ends, the selection is determined primarily by the supposed bearing of certain data on these ends. All the objects and facts deemed necessary for the construction of roads and bridges, the healing of diseases, or state control of human relations are studied together, to the exclusion of all such as are considered irrelevant to the purpose in question. In a theoretic science, like physics, biology, or comparative philology, the standard of selection is the possibility of continually extending and improving the abstract knowledge of objects and facts selected, so as to make it with every step empirically more exhaustive and logically more coherent.

In that part of scientific reflection about human life which since Comte has been called sociology, the standards of selection were originally practical, chiefly political and moral, and they have remained so in a very large measure. Social students are continually selecting and grouping together for comprehensive research the data that seem to them relevant for such ends as the prevention and control of crime, the welfare of the destitute, the promotion of harmonious relations between various races or classes within a territorial community, the substitution of peaceful understanding for military antagonism between states, and so on. For a long time, indeed in Europe nearly up to the present day, purely theoretic interest in social data was kept up mainly by philosophers of history and ethnologists; to most of those absorbed in solving practical problems of the concrete social world, sociology seemed either speculative or dealing with matters almost as remote from their vital problems as the satellites of Jupiter.

The knowledge thus agglomerated for practical purposes is by no means worthless theoretically. We owe to the ethical and political reflection of thousands of years, beginning with popular proverbs and the secret lore of savages and ending with modern works on the conditions and possibilities of social betterment, a store of sociological information which we have not even begun to appreciate. Sociology, like every ambitious upstart, is inclined to consider everything the past has left us in its domain as worthless dross. This is rank ingratitude, for as a matter of fact sociology still lives chiefly on the achievements of former generations; and it is very fortunate for it at its present stage that mankind already knows incomparably more about social reality than it knew about nature at the time

when the physical and biological sciences commenced their tremendous advance.

This vast mass of knowledge cannot, however, become a part of theoretic sociology until it is completely reorganized. For in a theoretic science all knowledge already achieved is deemed unsatisfactory and used only as an instrument to achieve more and better knowledge, whereas a practical science treats the knowledge it possesses as final as long as it serves its ends, and does not attempt to improve it unless forced by practical failure. The knowledge agglomerated in the course of practical pursuits thus remains scientifically unproductive, and nowhere is this more manifest than in the domain of sociology. The bulk of sociological generalizations has, indeed, been growing since antiquity, but only under the pressure of new practical needs and purposes. In those lines in which our conscious practical ends have remained essentially similar to what they were two thousand years ago, such as social control of private relations, education and politics, we have little better knowledge of the means than the Greeks and Romans had. Not until quite recently is a slight theoretic progress in these fields noticeable, and this is entirely due to the beginnings of positive and disinterested sociological research, however hesitating and imperfect these beginnings may yet be.

Since the whole content of a science obviously depends on the data it studies, the first task of those who started to build a theoretic science of sociology in a field where formerly practical science reigned supreme —a task which was and still is rather neglected—should have been to change the standards determining the selection of the objects and facts to be investigated. The attainment of a practical end requires in most cases a

great variety of information which cannot possibly be included within the limits of one theoretic science, but must be dealt with by several special sciences. Engineers who build a bridge use a fund of general information to which physics, chemistry, mineralogy, geology, meteorology, economics, have contributed their respective shares. A physician who wishes to heal a patient must utilize knowledge whose theoretic sources lie within the domains of biology (subdivided into anatomy, physiology, bacteriology, cytology, etc.), psychology, chemistry. An educated farmer wields general truths belonging severally to botany, zoology, organic and inorganic chemistry, geology, meteorology, economics. Judging by these examples, it seems highly improbable that all the knowledge needed—say, to diminish crime or prostitution, to assimilate a foreign population, to raise the cultural or hedonistic level of a rural community, or to prevent wars between states—should pertain to one theoretic science, however comprehensive this science may be.

On the other hand, we find that a theoretic science always furnishes information to several distinct practical sciences, none of which utilizes all the knowledge that might be drawn from the common source. Thus, chemical knowledge is used in the dyeing industry as well as in agriculture, in the production of explosive and poisonous gases as well as in pharmacology and medicine. Similarly, theoretic sociology will be—and even now is beginning to be—used for the purposes of politics and practical ethics, education and business enterprise, peace and war, class struggle and class accommodation, urban and rural organization, etc. However, at every stage of its progress a theoretic science contains much knowledge that is not yet utilized by any practical

science. Some of it will be used sooner or later, but in the meantime theory will have reached new results and again practice will be lagging behind. A certain amount of theoretic knowledge may even remain for ever practically useless, though indispensable as a component part of the total body, the greater part of which sooner or later finds practical application.

No theoretic science can, therefore, afford to have the selection of its object-matter prescribed to it by any practical considerations. It may, indeed, and often does undertake to study problems suggested by practical science, but these problems must lie within its field as circumscribed by theoretic criteria. There is no reason why a sociologist should not at the instigation of a statesman, a moralist, a social reformer, study some of the data involved in peace or war, in contacts between races, in crime, poverty, class struggle or family disorganization, in order to reach conclusions which will help the practical scientist to outline plans for social betterment. But he must have definite theoretic standards to judge which of the innumerable and varied facts bearing on a given practical end belong to his proper domain; and he must limit himself to these. Everything outside must be left to other specialists for study, while the task of unifying and organizing the results reached by the various specialists belongs to the practical scientist, the "social engineer."

Theoretic sociology being still, as its own adherents confess, much less efficient in its research than many older and more developed sciences, it should obviously try to benefit by their example in establishing the standards for selecting its data out of the enormous wealth and complexity of human experience. In this matter the actual process of scientific procedure ought

to be followed rather than the abstract principles laid down in the works of logic, for precisely here logical theory is still inadequate, its great modern development being chiefly concerned with so-called deductive rather than inductive sciences.

2. *Concrete Reality Inexhaustible*

Every inductive science pursues two main purposes, whose nature and connection will be more exactly characterized later on: it attempts, first, to describe accurately certain kinds of empirical objects; secondly, to explain certain kinds of empirical changes or processes ("facts," in the exact sense of the term). Now, it must be clearly and definitely understood that no particular concrete object or agglomeration of objects as it appears to our uncontrolled observation can be adequately described; no particular concrete process as it happens to occur within our undirected experience can be explained. We know that physics deals with inanimate objects and processes, biology with organisms, philology with language. Suppose I were to ask a physicist to describe scientifically a particular stone or heap of stones on the road, or a biologist a tree or forest growing near the road, or a philologist my conversation with him or even one sentence of that conversation. Each of them would begin by pointing out certain features of these objects, but other features would then obtrude themselves on my attention, then others still, until both he and I would be overwhelmed by a mass of empirical characters. The description would prove an infinite task, becoming more difficult with every step. Of course, my scientific companion would claim that these characters are not equally important from the

point of view of his science, but unless I knew and accepted the special theoretic criteria by which he appreciates their relative importance, his claim would entirely fail to persuade me: as a casual observer, I should see no reason for reducing the description to some characters of the object and ignoring the rest.

Suppose, further, I asked a physicist to explain such an apparently simple process as the formation of centrifugal waves on the surface of a pool of water after I had thrown a stone in, a biologist the fall of a particular leaf from a particular tree at which we happened to be looking, the philologist the defective pronunciation of a word by a particular person. They would probably again begin by referring to some definite cause or set of causes, presuming I knew and accepted the criteria that justified them in stopping their explanation at a certain point and being satisfied with it without trying to go any further. But if I insisted on a full explanation, seeing as a naïve observer no reason to consider some of the factors contributing to the given happening as direct and essential, others as indirect and secondary, they would obviously have to give the problem up. For, to take into account every fact that was necessary to have waves of this particular shape, length and duration appear on the surface of this particular pool at this particular moment, we would have to know practically everything about the past of the pool, of the stone I threw and of my own self; a complete explanation of the fall of this particular leaf at this moment would require an exhaustive knowledge of the history of the leaf, the tree, the breeze that happened to blow, the soil, the climate of the country, the genesis of this arboreal species. The mispronunciation of the word could be fully explained only

if we were thoroughly acquainted with the history of the language, of the organism of the man who used it, of his racial stock, of his cultural milieu.

And yet such is, strictly speaking, the task a sociologist unconsciously undertakes when, without other scientific preparation than a keen sense of reality, a vivid interest in facts, some previous training in observation of the same type, and some reading of works in which observers similarly prepared have set down their results, he starts to describe a given family in a particular neighborhood, the Chinese people in an American city, or the personality of an individual leader; to explain the Bolshevist Revolution, a miners' strike, or a case of youthful maladjustment.

Often, as a matter of fact, he does much better than might be expected under such conditions. He does not attempt to describe or to explain anything as exhaustively as he should if he were as conscientious an observer as he claims to be: practical considerations, social tact, aesthetic interest, and sometimes sheer laziness luckily force him to limit his description to certain characters that appear to him as most relevant, to draw for explanation only upon certain causes which seem to him most important. Nor is his choice of relevant features and important causes as arbitrary as it would be if he were as unprejudiced an observer as it is his ambition to be. He has very definite, though seldom rationally motivated, presuppositions concerning the comparative relevancy of various features of the objects he means to describe and the relative importance of the different kinds of causes he intends to search for. These presuppositions are a part of that practical sociological knowledge we have inherited from our predecessors, in some cases modified and improved by

recent theoretic reflection. They are not worthless, any more than the rest of this knowledge is, but they need to be made more explicit and be critically surveyed, standardized and systematized for theoretic purposes. For only too often the sociologist's choice of relevant characters is like that of the boy who thinks the most important character of the stone is that it can be thrown, and of the tree that it furnishes wood, while his causal explanations bear a marked similarity to those of the rustic who says the leaf fell because it was withered, and the man mispronounced a word because he is of foreign origin.

The problem of selecting scientific material is thus in every science bound up with the problem of discriminating between relevant and irrelevant characters of the objects, important and unimportant causes of the processes selected. However carefully we choose the objects and processes to be exclusively investigated by our science, this will avail us nothing if every step of our investigation must lead us deeper and deeper back into the inexhaustible and chaotic wealth of concrete empirical reality. Every successful and advanced science has not only its material limited by objective criteria of selection, but also the use which it means to make of this material determined by objective standards of relevancy and importance applied to the characters of its objects and the conditioning antecedents of its processes.

3. *The Principle of Closed Systems*

All these criteria and standards seem to be based on one fundamental assumption which we find explicitly or implicitly guiding the physicist and the astronomer,

the chemist and the geologist, the biologist and the philologist, the economist and the art student, in the choice and determination of their respective data. It is the assumption that reality is constituted by innumerable and various *closed systems*, that is, systems each of which is composed of a limited number of elements more intimately inter-related with one another than with any objects which do not belong to the system, and each possessing a specific internal structure which isolates it in certain respects from external influences.[1]

Whether any given concrete object or agglomeration of objects appears to the scientist as a system or as an element depends on his "point of view," i.e., on the kind of problems he means to put and to try to solve. Thus, the sun together with all its planets may be viewed as one element of a wider sidereal system, but the sun and all the planets including the earth may be considered as elements of the solar system. To the

[1] I took the concept of closed systems from the French methodologists who were grouped around the *Revue de Métaphysique et de Morale* about twenty years ago, but I have since then modified it and developed its applications. In the meantime, physics has discarded that particular type of closed system which was the original source of this concept, viz. the energetic system as determined by the first two laws of thermodynamics. But that does not affect the validity of the principle of closed systems as such. There is no logical reason why, for instance, a closed system should not generate energy, provided it does it in accordance with some law; nor why it should be regarded as absolutely isolated from external influences, provided these influences can be determined with reference to its composition and structure. Indeed, it is only with reference to a closed system that such facts can be determined, or even discovered.

We leave for later discussion the problem of substitution of systems of functionally related processes for systems of interconnected objects.

geologist the earth appears as an extremely complex system; yet within the masses of matter treated by the geologist as elements which are combined to make the structure of the earth, the mineralogist, the chemist and the physicist find each innumerable and varied systems, until the analysis reaches the physical atom as a system of electrons. To the phytosociologist ("plant sociologist"), a particular plant is an element of a system; to the botanist interested chiefly in the description and classification of plants, it appears as a system of organs; further analysis dissolves each organ into a system of cells, and for the student of cytology each cell is a system of plasmic elements and processes.

Every particular empirical object in the course of scientific research is thus viewed either as an element of a system to be understood only in its relation to the other elements and with reference to the structure of the system as a whole, or as a system to be understood only with reference to the elements of which it is composed and which are combined in its structure. The first task of the scientist is to *circumscribe* the system as closely as possible, to determine what elements belong to it. Though in some cases this seems easy and obvious, as for instance when the zoologist studies a higher animal as an anatomic system, yet frequently much preliminary research is needed before this task is completed. It took many centuries of observation and calculation to separate the planets from among the thousands of brilliant points visible on a starry night, and many more to discover the full composition of the solar system. Special tests are often needed to distinguish a chemical compound from a mere physical mixture. To be perfectly sure what elements belong to a system we must know what are the particular connec-

tions binding them to one another within the system, for only then can we tell whether any given object is or is not thus connected with the rest.

4. Description of Systems and Elements

The preliminary problem of circumscription therefore partly overlaps the first fundamental task of scientific research: that of *description.* When dealing with a system, we need to select for study only those objects which belong to the system as its elements; we are no longer faced by a bewildering mass of heterogeneous empirical data without any possibility of describing all of them nor any guidance as to their choice except our arbitrary interest, but we know that there is only a limited number and variety of definitely interconnected objects which must be taken into account. While this criterion permits us to ignore most of the things which thrust themselves upon our undirected attention, it forces us on the other hand to search for things which escape superficial observation and yet must exist as elements of the system, as our study of the other interconnected elements leads us to conclude. A classical example to the point is the discovery of the planet Neptune after its existence had been inferred from perturbations in the movements of Uranus.

If the system as circumscribed at the outset should prove too comprehensive, the number of particular objects too large for scientific description, the scientist will either try to break it up into smaller systems and study these, or else group many objects together into larger units which behave in certain respects as elements. The first procedure is exemplified by modern physical investigations of the composition of matter; instances of

the second method are found in plant sociology, which is beginning to treat as elements of an ecological system no longer the individual plants, but all the plants of a species living within the given territory; or in human geography, whose units are not individual men, but more or less durable agglomerations of men like towns and villages.

Furthermore, the particular objects thus selected for study on account of their belonging to a system are not taken in their total and inexhaustible empirical content, nor is their description arbitrarily limited to any features which happen to strike the observer as relevant because they appeal to his practical interests or to his aesthetic imagination, or simply because he has acquired the habit of looking for them. The fact that an object belongs to a system gives a definite scientific standard by which the relevancy of its characters must be judged. All those and only those of its characters are scientifically relevant for the purposes of any actual investigation which determine its position and function within the system with reference to which it is being investigated. Thus, for a physicist who describes a system of bodies in movement the relative position of each body with reference to the others at definite moments of time, its mass, the velocity and direction of its movement are the essential features to be taken into consideration, while he not only can, but must ignore for his present problem all the other characters of this object, e.g. its color, its taste or smell, which an observer who has no such problem to solve may discover. The chemist who describes a compound characterizes each of the elements of this compound with regard to those of its "chemical properties" which are relevant for the rôle the given element plays in the compound.

While thus ignoring as irrelevant for his actual scientific task most of the features of an object which seem important to the "unprejudiced" observer or to one who is prejudiced by practical interests, the scientist is often forced to search for characteristics which cannot be discovered on superficial observation and yet without which the connection of the object with other objects of a system would be incomprehensible. The whole successful modern investigation of the electromagnetic properties of material reality, which were almost completely unobserved in the past, is perhaps the best case to the point.

5. *Explanation of Changes*

A system is relatively isolated from external influences owing to its structure, i.e. to the total combination of forces which keep its elements connected in a way none of them are connected with any outside objects. Thus, the structure of the solar system, i.e. its specific combination of centrifugal and centripetal forces, holds it together as a separate entity within the sidereal world; the anatomical and physiological structure of the organism permits it to avoid such influences of the milieu as would otherwise disrupt it. In so far as a system remains structurally unchanged, that is, undergoes no modifications except such as result from the very forces involved in its structure (such as the changes in the relative position of the sun and the planets resulting from the forces in action within the solar system itself, and the periodical changes due to the normal functioning of the organism), all our knowledge of the system takes the form of scientific description. But as a matter of fact systems do

change structurally in ways which are not the outcome of their internal forces alone, but are due to disturbing outside influences. No system is totally cut off from outside reality, because none of them determines completely and exclusively the nature and relations of all its elements. Elements of a system are concrete objects which not only possesses other characters besides those defining their rôle within this system, but are also subjected to other influences besides those involved in the structure of the system. The earth has features which are independent of the rôle it plays in the solar system (for instance, there are processes going on within it independently of its actual connection with the sun and the other planets), and it is subjected to influences from heavenly bodies outside this system. The particular organs, tissues and cells of a complex organism are not entirely and exclusively elements of it, but each has a minimum of independent life of its own; and there are many forces outside the organism which may and do affect it at any moment. Consequently, the elements of a system do change under the influence of factors which are not involved in the structure of the system, but are either independent processes going on within each element or else processes occurring in the outside world with which the given element is in touch and of which in some respects it remains a part, even while in other respects it belongs to the given system. Many of the changes which an element undergoes do not affect its rôle in the system, or affect it in such a slight measure as to remain negligible. Thus, it seems that neither internal telluric processes nor the electromagnetic influences of other stars than the sun have markedly affected the rôle of our earth in the solar system in the course of a million years or so. But there are many cases

which show that the change of one or several elements exercises a disturbing influence on the system as a whole and may even modify its entire structure. Endogenous modifications of endocrinal glands or a wound in the head may affect the whole physiological and even anatomic structure of a living body; a new factor introduced into the closed system artificially constructed by the physicist in his laboratory for the purposes of an experiment modifies the functioning of the system.

Now, such a change in the structure of the system, since it is not implied in this structure, is not included in the original knowledge of the system which we have gained by the study of its elements with their connections and expressed in an exact rational description. It is something new and unexpected, something irrational which must be *explained*, that is, rationalized secondarily. This is done by conceiving it as the *effect* of some *cause*.

No successful science attempts nowadays to explain any process happening in empirical reality at large otherwise than by analyzing it into changes of definite closed systems exactly circumscribed and described. Thus, the spread of an epidemic disease is studied by taking particular cases and investigating the processes of contagion and incubation, development and cure of the disease with reference to particular human organisms. No serious student would think of explaining the epidemics by merely observing other processes which in the given anthropo-geographical area preceded and accompanied its spread and decrease, even if these processes have been statistically formulated; at best such an observation would sometimes suggest to him the possible influence of factors which otherwise he might overlook in studying individual cases. The kind of

reasoning which searches for the causes of a concrete process of epidemics in antecedent or contemporary concrete processes within the same area has, however, been in use for innumerable centuries and has led to very curious theories, such as that which explained epidemics as the effect of the sins of the people so afflicted.

Only when we are able to connect a process with some closed system and interpret it as a change of this system do we gain a standard of the relative importance of its antecedents as factors in its occurrence. The first step of our explanation is then necessarily limited to the system itself; the first cause of the change of the system must be sought in a change of some of its elements which did not result from the forces involved in its structure. A machine which worked once ceases to work: we presume that the determining cause is some change in its elements. Some essential part may have dropped out or been broken, a strange body has penetrated between the parts, or a chemical process of oxidation has been going on. The leaves of a plant wither: this may be due to the lack in its composition of some elements necessary for the nourishment of the leaves, to some poison which has penetrated into the plant, or to the destruction of some roots which are indispensable to the normal functioning of the plant as a system.

Once this change of elements has been exactly ascertained, the change of the system is causally explained and there is no logical necessity for proceeding any further; the explanation, though limited, is perfectly satisfactory as far as it goes. And until this first problem has been solved, there is no use putting any other problems. Suppose, however, we are not satisfied and wish to know what has brought into the system this change of an element that was the cause of the effect we started

to explain: why did that part of the machine fall out or get broken, why did the strange body come between the parts, why did the plant lack nourishment, what was the cause of the destruction of its roots? How can we avoid the indefinite complexity of concrete antecedents and limit our research to certain possibilities, while safely excluding others? Again we appeal, consciously or not, to the principle of closed systems. We assume that the original change which occurred within our given system and caused its final modification was due to the fact that the particular element (or elements) which has been changed is (or was) also an element of some other system, and we try to trace its change back to some antecedent modification of this other system. For instance, the breaking of a part of the machine may be explained by showing that a strain was put upon it by the weight of some heavy outside body: this means that the part of the machine in question was for a time an element in a system of statics of which the outside body and the earth were other elements. The oxidation of the iron parts of a machine appears as the result of those parts having been elements of a chemical system which included also oxygen and hydrogen from the atmosphere; the chemicals necessary for the nourishment of a plant were parts of the environment of the plant and a change in this environment has resulted in their withdrawal from the reach of the plant. And so on.

After two changes within the same system or two interfering systems have been thus causally connected we can try to formulate the connection in terms of *functional dependence*, if indeed both the cause and the effect are processes which show some kind of continuous (or even certain types of discontinuous) quanti-

tative gradation. The functional relationship is thus in inductive science not a substitute for the causal relationship, as some logicians absorbed in the mathematical ideal are inclined to think, but merely a more perfect, i.e. more exact and more certain kind of causal relationship. This, however, is a problem to which we shall return later.

6. *Historical Knowledge as Opposed to Generalizing Knowledge*

There are branches of research like the geography of Europe or the political history of England, whose main purpose is to describe some particular individual systems in their concrete setting or particular combinations of interconnected systems and to explain the particular changes to which these have been subjected in the course of their existence. There are other lines of investigation prevailing, for instance, in physics, biology, or comparative political science, where the description of any particular system or the explanation of any particular change is merely the first stage of a wider scientific task which consists in obtaining an abstract definition applicable to all the systems of a certain class, or in reaching a general causal law applicable to all the changes of a certain kind. On this ground distinctions have been made between idiographic and nomothetic, or historical and natural sciences.[1] However, we do not think the difference deep enough to justify such a separation.

Historical description and explanation of any particular portion of concrete reality cannot reach a scien-

[1] These distinctions are due in the main to Windelband and Rickert.

tific level without abstract definitions and general laws. For, as we have seen, concrete reality in its empirical complexity and wealth is undescribable and inexplicable. And if we wish to circumscribe any particular empirical system in itself, to distinguish it from its empirical setting, or to find the interrelated systems combined in a given portion of reality, we can seldom be sure which elements do and which do not belong to any system we have in mind—unless we have constructed it ourselves or reconstructed it in accordance with some model already known. The innumerable and varied connections between empirical objects are here an ever-present source of difficulties and errors. This is one of the main reasons for experimental synthesis: the experimenter in physics, chemistry, biology, constructs a closed system artificially; and, of course, apart from any technical mistakes he may make, he knows what elements belong to it, since he has included them himself to the exclusion of others.

Where experiments are impossible, the only way of testing an analysis of the composition of a system is to compare it with others of a similar structure. For it is a fact, and a lucky fact for science, that there are usually many systems of a certain type of structure in the world—many planetary systems, magnetic fields, chemical compounds, organic cells, with similar combinations of forces holding the elements together; and where the structure is similar, the composition is not apt to differ beyond certain limits of variation. Even in sciences which use experimental constructions comparisons are indispensable: the physicist or biologist who builds a system in the laboratory means to discover features which will be applicable to the reality outside the laboratory. It is this external, natural reality he

wishes to know, not the one he makes artificially. He explicitly or implicitly assumes that his system stands for a certain class of empirical systems existing independently of him, though rather difficult to circumscribe, that it is a standard system with which empirical systems will have to be compared whenever we wish to circumscribe and describe them exactly.

Equally difficult is the explanation of one particular change occurring in a system. Even if the system is well circumscribed and its composition and structure known, its elements may be subjected to many and various modifying influences. Hence it must always remain a doubtful question which one of these modifications is the determining cause of the change of the system we wish to explain, unless we can compare this change with other changes to which the same system or other systems have been subjected and from among their varying antecedent changes of elements select the one which invariably precedes the same kind of change in the same kind of system and never precedes a different one. Here again experiment is a great help in so far as it permits us to limit at will the number and variety of antecedent changes, and thus is facilitated the discovery of the cause to which the effect to be explained is directly due. Still, far from allowing us to dispense with comparison, experiment is founded upon it, not only in the sense that several experimental processes must usually be compared to reach a valid explanation, but that its whole scientific significance lies in the possibility of comparing empirical processes going on in the world outside the laboratory walls with the processes experimentally produced within.

Unless, therefore, historical investigation is satisfied with results whose scientific value must remain for ever

doubtful, or unless it is willing to apply aesthetic or moral, instead of theoretic standards and thus resign all claims to be a science,[1] it must appeal for help to classificatory and nomothetic research. This is done in two ways. If the particular object-matter of historical investigation can be viewed as a single system, it is compared with other systems and a search is started elsewhere for processes similar to those occurring within it, so as to obtain a more certain knowledge of its structure, composition and changes. Thus, geologists have been making comparative studies between the earth and other heavenly bodies on the one hand, telluric processes and processes artificially produced in laboratories on the other hand. Political historians in studying a particular state compare it with other states. If, as more frequently happens, the historian is interested in a portion of reality which must be conceived as a combination of several different interconnected systems, then he compares each of these with other systems of the same variety found in other combinations. The given portion of reality thus appears as a particular synthesis of these typical components, and the complex process which goes on within it is viewed as the resultant of various elementary changes, each of which can also occur elsewhere and is always subject to some causal law. The concrete combination may be unique, but its constituent parts are representative of more or less general classes. Such is the tendency of the modern geographer dealing with a particular area or the trained ethnologist studying a particular tribe.[2]

[1] Some philosophers defend and even advocate æsthetic and moral standards, particularly in the history of culture. But sociology has passed beyond that stage.

[2] See, for example, *Methods in Social Science*, Sec. IV, "Attempts

While historical knowledge to be scientifically valid must, therefore, be controlled by classificatory and nomothetic knowledge, the latter is obviously dependent on historical data. Not only is the study of particular systems and individual changes a necessary condition of all inductive generalizations, but the ultimate significance of these generalizations is that they help understand such particular systems and explain such individual changes as have not yet been investigated. When the scientist has learned the typical structure and composition of systems of a certain variety, his result is not a Platonic idea to be contemplated in detachment from the empirical world: it is an instrument for the analysis and understanding of new fragments of concrete reality which come under his observation and for the discovery of varieties of systems yet unknown. When he has formulated a causal law concerning a certain kind of change, he is not to rest on his laurels, treating the law he has promulgated as an established dogma, but to use it for the analytic study of other empirical processes and the discovery of new laws.

Historical knowledge and generalizing (i.e. classificatory and nomothetic) knowledge thus differ merely in the relative emphasis they put upon the one or the other of the two essential and complementary directions of scientific research: in both cases we find a movement from concrete reality to abstract concepts and from abstract concepts back to concrete reality—a ceaseless pulsation which keeps science alive and forging ahead. Whenever historical knowledge is separated from generalizing knowledge, it is because the merely practical division of labor between scientists is unduly

to discover spacial distributions and temporal sequences," pp. 238-283.

treated as equivalent to a logical distinction between sciences. We see this, for instance, in the organization of universities and institutions for research under the influence of old and rationally unjustifiable traditions. It is the traditional respect for "erudition" in the sense of a thorough and detailed acquaintance with many particular individual data, which constitutes the main reason why such branches of study as "Zoology," "Greek Philology," "Egyptology" and "American History" are treated as separate sciences. In view of the enormous wealth of scientific material, it is, of course, desirable that some people be exclusively occupied in collecting positively established data and making them available for scientific study; but this is technical, not scientific work, just as the technical preparation of colors for the painter is not identical with the artistic activity of painting. Usually scientists still have to do this preparatory labor for themselves, as the painters of the Renaissance manufactured their own colors, simply because this branch of technique has not yet been recognized as a distinct specialty (except perhaps in museums). The resulting absorption in concrete data as such, apart from their significance for scientific generalization, encouraged by the old cult for erudition, has been perhaps the greatest check on scientific progress in sciences which draw their material chiefly from the past, just as the prevalence of practical over theoretic interests has hindered the advance of research bearing on the present.

7. *Facts and Theories in Sociology*

There is a special reason for emphasizing this point while discussing the selection and utilization of scien-

tific material in sociology. Up to the end of the last century sociologists were chiefly interested in reaching as quickly as possible wide and highly abstract generalizations. Not only did they utilize almost exclusively material predigested by other scientists, but they helped themselves freely to ready-made generalizations found in the literature of zoology, anthropology, psychology, history, ethnology, economics, philology, etc., selecting them with reference to the possibility of an all-embracing synthesis and never testing their truth, which indeed they could hardly attempt to do, not being specialists in the various fields they reviewed. If they did sometimes use concrete data, it was rather to support or illustrate their theories than to test them in good faith.

Now the pendulum has swung almost as far in the opposite direction. In many sociological circles nothing seems to matter but facts. Enormous masses of undigested concrete observations are being piled higher and higher with but little thought of ever utilizing them fully for an abstract and systematic knowledge of the logical type of physics, chemistry, general biology, or comparative science of language. Following the example of the conscientious monographic historian and the post-evolutionary ethnologist, the sociologist has at last gone for his data directly to the sources. Driven by pricks of conscience for having neglected so long his duty as inductive scientist, and perhaps also somewhat influenced by the common weakness for the anecdotic and the picturesque, sociologists are making enormous "surveys," describing particular communities and neighborhoods, agglomerating first and second hand information about nationalities, races, classes and their intricate relationships, issuing thou-

sands of questionnaires, collecting billions of personal and family documents, and interviewing millions of people. A critical study of the scientific use made of some typical collections of material for the purposes of formulating and testing general sociological hypotheses shows an appalling waste of good material and valuable energy. Moreover, the present-day sociologist in his passion for concrete research usually forgets completely all that concrete material which has already been agglomerated by men who, though ignorant of sociology, were at least his equals in keenness of observation and human interest.

And along with this new and purely historical trend of research, there survives still the old type of sweeping generalization, sustained by the undying human longing for a synthetic interpretation of the world. The two currents, the historical and the generalizing, instead of blending together, run in opposite directions: the "speculative" and the "empirical" schools keep up a continuous fight against each other, though all the time the former is slowly dying of inanition, and the latter is so surfeited with raw material that it is suffering seriously from indigestion. Even if the fact worshippers are right when they claim that the data collected in the past are incomplete and that sociology needs more concrete research of the historical type, their claim must be validated by the theorists. For the limitations as well as the advantages of the material already available can be ascertained only by using it to formulate and to test general scientific hypotheses; and the need for new material depends on the general scientific problems we want to solve.

There is only one course open for sociology, if it wishes definitely to avoid both the Charybdis of theoriz-

ing with no firmer ground than the hypotheses of the other sciences accepted as dogmas, and the Scylla of an irrational mass of motley information, however interesting in itself. This course is *to determine exactly the general type (or types, if there be several) of those closed systems which it is the special right and duty of the sociologist to study*, and to concentrate primarily on these, as consistently as physics, chemistry, biology, philology concentrate on their respective object-matters. It may prove eventually that some of these systems often or prevalently are components of wider and more complex systems, just as certain chemical systems are usually components of biological organisms; and it will surely be found that between those systems which the sociologists investigate and systems belonging to the domains of other sciences, there is frequent interaction, since all of them are only relatively closed and none absolutely impermeable to external influences. Such phenomena make the collaboration between sociology and other sciences desirable; but collaboration is only possible if each collaborator knows his own task.

Seeing that sociology has not yet been fully engulfed in the bottomless pit of speculation nor broken up on the rocks of disjoined "hard facts," we may conclude that it is already steering the proper course, however slowly and hesitatingly; it needs only to become fully confident of its direction and cease being lured away on various useless and dangerous enterprises.

References

Practical standards of selection of data in the social domain are well illustrated by the three famous works of

classical antiquity: Plato's *Republic*, Aristotle's *Politics* and *Nicomachean Ethics*. A good idea of the expansion which the field of practical knowledge subservient to political purposes has undergone during the last twenty-two centuries can be obtained by looking over the pre-war editions of the *Handwörterbuch der Staatwissenschaften* (recent editions include the wider and more theoretic concept of *Sozialwissenschaften*). Ethics has expanded much less; still, much heterogeneous theoretic information, logically belonging to biology, psychology, sociology, economics, the theory of the state, the theory of religion, and the theory of knowledge is needed to substantiate the generalizations included in such works as H. Sidgwick's *The Method of Ethics* (London, 1900), W. Wundt's *Ethik* (Stuttgart, 1903), or even a textbook like Dewey and Tuft's *Ethics* (New York, 1913).

In modern times, several new "practical sciences" have developed which are connected with sociology, but require the cooperation of many other theoretic sciences. Criminology is a notable example; see, e.g. one of the latest textbooks, T. E. Haynes' *Criminology*, New York, 1930. "Theory of Education" is another of these complex practical disciplines; even that part of it which has recently been separated under the name of "Educational Sociology" (as exemplified by the works of David Snedden, *Educational Sociology*, New York, 1924, and *Toward Better Educations*, 1931) shows a wide range of scientifically heterogeneous problems. There is a tendency to create a new practical science on the basis of facts and problems encountered in social work: Mary Richmond's *Social Diagnosis* (New York, 1917) is an interesting attempt in this line. Howard W. Odum's *Man's Quest for Social Guidance* (New York, 1917) indicates clearly the many various and theoretically unconnected sources from which the

knowledge for "social guidance" must be drawn, and the impossibility of organizing all this information theoretically.

On the other hand, attempts to outline the specific applications which can be made of sociology alone as one of the theoretic sciences to various fields of practice are relatively scarce. The term "Applied Sociology" was introduced by Lester Ward in the well-known work under that title (New York, 1906), but his conception is altogether too ambitious for practical purposes. Frederich R. Clow, *Principles of Sociology with Educational Applications* (New York, 1920), was probably the first to draw, modestly but effectively, special technological conclusions from sociological theory in a limited field. W. Adamski, *Outline of Applied Sociology* (in Polish, Vol. I, Poznan, 1928), is trying to organize systematically all the possible applications of sociology as a special science for guidance in practical life.

The problems sketched in sections 2, 3, 4, and 5 of this chapter have been widely discussed in modern philosophy, particularly (though not exclusively) in connection with the presuppositions of physical sciences. Of the many works under the influence of which the author's views have been shaped, a few may be mentioned here:

Boutroux, *De l'idée de loi naturelle* (1895).
Wilbois, "La Méthode des sciences physiques" (*Revue de Métaphysique et de Morale*, 1899).
——— "L'esprit positif" (*Revue de Métaphysique et de Morale*, 1901).
Poincaré H., *Science et hypothèse* (1902).
——— *La valeur de la science* (1905).
——— *Science et méthode* (1909).
Rey, Abel, *La théorie de la physique* (1906).

Bergson, Henri, *Essai sur les données immédiates de la conscience* (1889).
——— *Matière et mémoire* (1896).
——— *Evolution créatrice* (1907).
Le Roy, E., *La philosophie de Bergson* (1912).
De la méthode dans les sciences (collective work), Paris, Flammarion.
Goblot, E., *Essai sur la classification des sciences* (1893).
Naville, Adrien, *Nouvelle classification des sciences* (1901).
Meyerson, E., *De l'explication dans les sciences* [2] (1922).
Mach Ernst, *Populärwissenschaftliche Vorlesungen* (1898).
——— *Erkenntnis und Irrtum* (1905).
Wundt, Wilhelm, *Logik* [3] (1906).
Jevons, W. S., *Principles of Science* [7] (1900).
Pearson, Karl, *The Grammar of Science* [2] (1900).
James, William, *A Pluralistic Universe* (1914).
Dewey, John, *Experience and Nature* [2] (1926).

I am not sufficiently acquainted with the most recent developments of the philosophy of physical sciences (outside of such well-known works as those of Jeans and Whitehead) to follow their general methodological implications. And, in any case, the type of scientific problem with which this new methodology is dealing is already far ahead of the possibilities of present-day sociology, which can derive more advantage from the study of older physical and biological methods.

A good book for the student who wants to avoid the one-sidedness of uncritical empiricism is Morris C. Cohen's *Reason and Nature* (New York, 1931), though the author's treatment of social sciences is not quite adequate.

On the relation between historical and generalizing knowledge, see

Dilthey, W., *Einleitung in die Geisteswissenschaften*, I (1883).
Windelband, W., *Präludien* (1884).
——— *Geschichte und Naturwissenschaft* (1894).
Rickert, H., *Die Grenzen der Naturwissenschaftlichen Begriffsbildung* (1896).
Croce, B., *Il concetto della Storia* (1896).

There are many important monographs on this subject in the *Revue de synthèse historique.*

The difficulties of scientific selection of object-matter in sociology and the neighboring fields are well illustrated in the collective work *Methods in Social Science*, edited by Stuart A. Rice (Chicago, 1931). Sorokin, P., *Contemporary Sociological Theories* (New York, 1928) implicitly ignores the need of circumscribing any special field as the object-matter of sociology, and even denies it explicitly in several places (Cf. p. 506). Ellwood ("Emasculated Sociologies," *Sociology and Social Research*, XVII) radically opposes all such attempts.

CHAPTER II

The Principles of Selection of Cultural Data

1. *The Distinction between Natural and Cultural Data*

A comparative survey of the closed systems with which various special sciences have to deal shows a fundamental difference between two main types of systems: the *natural* and the *cultural*. The difference concerns both the composition and the structure of the systems, and the character of the elements and of the forces which bind them together. The distinguishing features are by no means hidden from observation and to be discovered only after a long process of research: on the contrary, they are given at the outset of the study; they determine from the first moment the direction of all further research; and it is their very obviousness which, as often happens, has made many methodologists and philosophers ignore them in attempts to create an artificially monistic conception of science.

The difference concerns the part which human experience and activity play in the real world. And here we must settle once for all a much discussed point. What we know about reality, we know only by experiencing it and actively thinking about it; that being so, human experience and activity, i.e. the experience and activity of the investigating scientist, are ever

present factors in the study of all real systems. The idealistic philosopher will say that it is the determining factor, that all reality is only what the scientist makes it; the empirical realist assumes, on the contrary, that the scientist does nothing except discover what exists independently of him, that his experience and thought do not affect in any way the reality he investigates, but merely serve to introduce him to it, as it actually is. This conflict of philosophic opinions does not interest us here. The attitude of the positive scientist, the specialist in any field, is uniformly realistic. He always means to learn as exactly as possible about reality such as it is, independently of him. Whether his object-matter be nature or culture, he eliminates himself entirely, tries to behave not as a human being who wishes reality to accord with his particular prejudices, but as an impersonal "knower." If he notices that his experience and activity do affect his data, he treats this as a source of error to be avoided. Thus the astronomer corrects his "personal equation" in observations, and the psychologist or sociologist tries not to influence personally the people whose behavior he studies. In short, all science tends alike to approach complete objectivity. The difference in question does not lie in the attitude of the scientist but exclusively in the character of reality itself as given to the scientist when it is made the object-matter of impersonal investigation.

Natural systems are objectively given to the scientist as if they existed absolutely independently of the experience and activity of men. The planetary system, the geological composition and structure of the rind of the earth, the chemical compound, the magnetic field, the plant and the animal, are such as they appear to the student, without any participation of human conscious-

ness; scientifically speaking, they would be exactly the same if no men existed (apart, of course, from the metaphysical problem which we have already left aside). The essential characters of their elements, i.e. those characters which determine their functions in their respective systems, are such as they are apart from the question whether and how anybody experiences them; they are bound together by forces which have nothing to do with human activity. Even if a particular system has been artificially and intentionally constructed by men, as the experimental system in a physical laboratory or the ecological system of plants cultivated in a certain area, the naturalist is not interested in the experiences and activities of those who made it, but only in the natural characters of the elements which have been used for its construction, in the natural forces which hold them together after the system has been constructed, in the natural processes which occur within it. Therefore he can treat the laboratory system as a typical instance of systems existing outside, without the participation of men, but not so easily circumscribed, and consider the ecological system of cultivated plants as a particular variation of "communities" of plants growing wild.

2. *The Humanistic Coefficient of Cultural Data*

Very different appear such indubitably cultural systems as those dealt with by students of language and literature, art, religion, science, economics, industrial technique and social organization. Generally speaking, every cultural system is found by the investigator to exist for certain conscious and active historical subjects, i.e. within the sphere of experience and activity of some particular people, individuals and collectivities,

living in a certain part of the human world during a certain historical period. Consequently, for the scientist this cultural system is really and objectively as it was (or is) given to those historical subjects themselves when they were (or are) experiencing it and actively dealing with it. In a word, the data of the cultural student are always "somebody's," never "nobody's" data. This essential character of cultural data we call the *humanistic coefficient*, because such data, as objects of the student's theoretic reflection, already belong to somebody else's active experience and are such as this active experience makes them.

If the humanistic coefficient were withdrawn and the scientist attempted to study the cultural system as he studies a natural system, i.e. as if it existed independently of human experience and activity, the system would disappear and in its stead he would find a disjointed mass of natural things and processes, without any similarity to the reality he started to investigate.

For instance languages, whether modern French or ancient Greek, exist only in so far as they are spoken and understood by the people using them, i.e., by a historical collectivity living in a certain area within a certain period, with the addition of some scattered individuals living elsewhere or at later periods; and they have for the philologist the characters they possess or possessed for that collectivity. Islam as a religious system exists only in so far as a certain wide and complex human collectivity in the East believes in it and follows its ritual; and it is viewed by the student of religion through the eyes of that collectivity, or as modified by particular sects and theological schools. The Bank of England as an economic system exists only in so far as numbers of people in

England and elsewhere perform certain economic activities and have certain experiences, owing to which "the Bank" has a reality and exercises an influence upon human life; the student of economics must take it as he finds it within the sphere of experience and activity of those people, with all it means to its shareholders, directors and employees, agents, correspondents, debtors and creditors. The Platonic system of philosophy means that system as it has existed within the sphere of mental activity and experience of Plato himself and of all his disciples, readers and critics from antiquity down to the present day, and must be studied by the objective historian of philosophy and science only in so far as it has been understood by all those people.

Suppose the student eliminates the humanistic coefficient: the French language then becomes an enormous and disconnected complexity of sounds pronounced through centuries by hundreds of millions of individuals belonging to the species *Homo Sapiens*, together with a still more voluminous and chaotic complexity of physiological processes going on within the bodies of those individuals; Islam or the Bank of England will appear as a still more bewildering chaos of sounds, bodily movements, physiological processes, piles of wood, bricks and mortar, masses of inkspotted paper; and the philosophy of Plato (unless the student is prepared to treat it as a superhuman, absolute system of pure, objective "Ideas") dissolves similarly into organic processes, sounds, volumes of paper, printers' ink. Within such an inexhaustible chaos of natural things and processes, the scientist may indeed find certain kinds of natural systems relatively closed, such as typical organic responses to certain classes of external stimuli; but these are as incommensurable with the

empirical reality originally found by the student of language, religion, economics, or philosophy as are the systems of chemical elements of which painters' colors are composed with a portrait by Gainsborough or a landscape by Turner.

3. *Values as Cultural Objects*

The humanistic coefficient concerns both the composition and the structure of cultural systems. Every element which enters into the composition of a cultural system is what it appears to be in the experience of those people who are actively dealing with it, and the student cannot know what it is unless he ascertains how it appears to them. The words used in the composition of a French poem are what they appear to be to the poet himself, to his listeners, readers, and imitators. The myths, verbal formulas, sacred implements, ritual gestures entering into the composition of a Mohammedan public ceremony exist as religious realities just as they appear to the believers who participate in this ceremony. The coin, notes, securities, bills of exchange, checks, etc., composing the assets and liabilities of a bank as an economic system are what they appear to be to the shareholders, workers and clients of the bank.

The scientist who wishes to study the poem, the ceremony, the bank, cannot approach any of their elements the way he approaches a stone or a tree, as a mere thing which is supposed to exist independently of any human being for all human beings to see who have similar sense organs: for if he tried to do so, the reality of the elements would escape him entirely and he would fail to understand the real rôle which they play within their respective systems. This rôle is determined not

merely by the characters these elements possess as natural things, but also (and chiefly) by characters which they have acquired in the experience of people during their existence as cultural objects.

No natural analysis can detect these characters. The student of culture can ascertain them in two ways: by interpreting what the people whose cultural system he is studying communicate, directly or indirectly, about their experiences with these cultural objects, and by observing their outward behavior with regard to those objects. These methods supplement one another and both must be used to obtain adequate knowledge. Thus, the musical quality and particularly the significance of the words of a poem, the non-material reality of a religious myth accepted by believers, the mystical force of formulas and gestures, the sacredness of implements of religious service, the economic power attached to little pieces of gold and printed paper, are as essential characters of these objects as their physical and chemical properties, and influence at least as much not only the thoughts and desires, but also the external, naturally observable behavior of the people. Indeed, they often influence it incomparably more. The partial destruction of a temple may not prevent religious ceremonies from being performed within it, but its desecration by an iniquitous deed which does not in any way change its natural properties makes public worship impossible within its precincts. For a bank the amount of economic power inherent in a purely ideal "sum" of money is much more important practically than the obvious and marked physical difference between gold coin and bills of exchange.

It is well to express by a difference of terms this essential distinction between natural objects, elements

of natural systems, and cultural objects, elements of cultural systems. We call natural objects *things*, cultural objects *values*, in view of their essential practical determination with reference to human activity.[1]

A value differs from a thing in that it possesses both a given *content*, which distinguishes it as an empirical object from other objects, and a *meaning*, by which it suggests other objects—those with which it has been actively associated in the past; whereas a thing has no meaning, but only a content, and stands only for itself. Thus, a word of some language has a sensible content —composed of auditive, muscular, and (in languages which have a literature) visual data; but it has also a meaning, i.e., suggests those objects which it has been made to indicate. A "sacred" vessel, as an implement of a cult, in addition to its content (visual, tactile, etc.) has a meaning in a particular religion, owing to the fact that it has been connected with certain words, myths, ritualistic gestures, human bodies as objects of sanctification, and suggests them when experienced. A coin, by content a piece of metal, has a familiar meaning called "buying power." And so on. Whereas a stone or a drop of water, as things, have no meaning, or at least are treated by the physicist who studies them as if they had none and suggested nothing beyond themselves. This distinction has nothing to do with any opposition of "subjective" and "objective" data. Only from the point of view of naïvely materialistic metaphysics (unhappily quite popular now in certain circles of social

[1] For a general theory of cultural objects, see the author's *Cultural Reality*, University of Chicago Press, 1919. I advocated the use of the term *value* as a logical category distinct from the traditional category of "thing" or "substance" in my book *The Problem of Values in Philosophy* (in Polish), Warsaw, 1910.

scientists and psychologists) does objectivity appear coextensive with sensory experience. A value is as objective as a thing in the sense that the experience of a meaning, like the experience of a content, can be indefinitely repeated by an indefinite number of people and consequently "tested." To experience a meaning, indeed, a certain preparation or "learning" is needed; the individual must be put into definite conditions and be taught how to use the given value. But the same holds true of experiencing contents: the reproduction of a sensory observation is only possible under definite conditions of the individual's organism and milieu, and requires a previous training. In another sense again we might say that things are as subjective as values, since the ultimate empirical test of the reality of both is actual individual experience which, as shown by illusions and hallucinations, is not a sufficient guarantee of objectivity and in both cases must be controlled by reflection. Moreover, psychogenetically, values seem more primary and fundamental than things: we begin our life by adapting ourselves to a world full of meanings, and only much later, under the influence of certain practical and theoretic considerations, some of us learn to treat certain objects at certain moments as if they were meaningless.

When a value is taken with reference to a particular system, it may appear as "desirable" or "undesirable," "useful" or "harmful," etc., in connection with the other values involved in it and from the point of view of its realization. We call this character of the value its positive or negative *axiological significance*. Thus, to the poet who tries to use a certain word in a sonnet the word has a positive axiological significance, if it appears aesthetically suitable, a negative significance in the op-

posite case. An instrument employed in a Christian religious ceremony is axiologically positive with reference to the Christian religion, but axiologically negative from the point of view of the Mohammedan cult. A sum of "money" has a positive significance for an estate, if it figures among its assets; a negative significance, if it is a part of its liabilities.

4. *The Problem of Human Activities*

What are, now, the factors involved in the structure of a cultural system which hold its elements together and isolate in some respects the system as a whole from the external world? The answer seems easy and obvious. It is human activity which has constructed the system by selecting its elements and combining them together to the exclusion of disturbing factors; it is human activity of a similar kind which actualizes the system again and again in a certain field of human experience and prevents it from being different each time it appears. A poem as a system of words has been built up by the poet, who selected and combined them in a certain logical and rhythmical order so as to produce a total aesthetic effect: every reader, reciter and listener reproduces this effect by repeating, either audibly or mentally, this combination; and while doing so not only tries not to drop any words or introduce any new ones, but intentionally excludes external noises and distractions as well as all psychological associations which might interfere with his rendering and enjoyment of the poem. A religious ceremony is generally constructed gradually in the course of many years by a succession of religious leaders, each adding some elements, excluding others, combining and recombining them so as

to obtain what seems the best religious result in the form of a certain mystical connection between the deity and the community of worshippers. Finally, the system becomes stabilized and sanctioned by tradition, and is afterwards reproduced again and again by priests and their faithful followers, with the same supposed consequences occurring whenever all the essential elements are there and no material or psychological disturbances are allowed to interfere with its reproduction. A bank is organized by the common activity of a group of capitalists and experts, the former getting together the capital stock, the latter planning and organizing the economic operations, including some things and excluding others, so as to reach the desired result in the form of a yearly dividend to the shareholders: it is maintained by the activities of the directors and employees, who systematically perform the operations as planned, utilizing the economic resources of the community by definite methods, and forestalling or counteracting all interfering external factors and even any disturbances which may possibly spring from their own private interests.

But the meaning of the term "human activity" is still rather vague and full of scientifically undesirable suggestions. We must give this concept a greater precision, since it is the pivot of all research in the domain of culture. Of course, experience is the only possible source from which scientific knowledge about any kind of human activity can be gained. The question is, how shall we utilize this source? For here, as well as in the study of empirical objects, elements of closed systems, there are two ways of approach. One is the way of the naturalist who, even while recognizing that cultural objects are human values and that cultural systems are

constructed by human activity, believes that human activity can nevertheless be studied as a natural process given to him (like other natural processes) without any reference to how it appears to anybody else; and also that a human value viewed in the light of a naturalistic theory of activity can be simply analyzed into a natural thing *plus* an equally natural process which goes on in the human being as a psycho-biological entity, and corresponds to this thing. The other way of obtaining an inductive knowledge of human activity would be to use consistently the humanistic coefficient in dealing with it and take it as it appears to the agent himself and to those who cooperate with him or counteract him.

This is not the place to describe and explain the historical evolution which has led to an almost exclusive predominance of the naturalistic approach in all attempts to reach by scientific induction a general knowledge of human activities—though it would make one of the most interesting and significant chapters in the history of science. The humanistic approach, on the other hand, is the usual approach of popular reflection dominated by practical interests, and has in fact remained current in historical interpretation and in all the special sciences of culture that study particular historical forms of literary, religious, artistic, economic, intellectual, political activity. But when it comes to a search for general principles and laws of activity, there is a powerful influence with which scientific induction has to contend in this line: the influence of traditional normative speculation. The humanistic point of view, the point of view of the active subject, has been used much oftener to establish general standards of human activities than to discover general truths about them.

Thus it came to pass that a disinterested inductive

search for general truths in these fields has become associated with the naturalistic point of view, not only in the eyes of the naturalists themselves, but even in those of many humanists and philosophers. "Naturalism" has come to mean not merely a particular way of approaching empirical data as independent of conscious and active beings, but in general the objective scientific attitude toward reality as against an attitude imbued with subjective valuation, respect for facts as against speculation that bends facts to fit preconceived ideas.[1] How widely spread this identification of naturalism with objective general knowledge still is, even among scientists dealing with culture, is manifest in two striking phenomena. On the one hand, we see the pitiful attempts of many prominent specialists (chiefly in America, England and France), great authorities in their respective fields, who are trying to assimilate second and third hand knowledge of the latest results of experimental psychology and biology in order to give their general hypotheses what they believe to be a firmer scientific foundation. On the other hand, the modern reaction against this current (chiefly in Germany) and against the whole naturalistic point of view tends to reject the method of scientific induction as developed by the sciences of nature and to substitute instead some special method.

Following in the wake of many better thinkers, I have subjected the naturalistic approach to general criticism in several works. I do not mean to do so in detail again, especially as I have found that scientists with a naturalistic trend of mind "never know when they are beaten." I shall, therefore, pass over the many

[1] Cf. Carl Rahn, *Science and the Religious Life*, Chap. IV, for this conception of the naturalistic attitude.

problems and pseudo-problems which spring from the conception of activity as a natural, psycho-biological process, leaving them to those who are positively interested in this kind of conception and its logical consequences. Only at the most important points I shall mark clearly the dividing line between the field of research circumscribed by this work and the fields of the biologist and the biological psychologist.

At the same time, and even more strictly, all connection must be avoided between sociological theory and normative speculation. Not that I wish to deny the importance of that kind of hierarchization of human activities with reference to some supreme norm or ideal with which in the social field ethics, philosophy of law, philosophy of education, and philosophy of social progress have always been concerned. Such a hierarchization will always be indispensable for the intellectual guidance of social activities in addition to that organization of knowledge for practical purposes which is the task of practical or applied sciences like political science, educational science, and the various disciplines used in social welfare and social reform. Every instance of practical organization of knowledge in any line implies the acceptance of a definite goal; and since human goals are multiple, changing, competing and often conflicting, the need for their standardization has always been patent, and always will be patent, to those interested in the practical control of human life. Normative philosophy satisfies this need.

But, while recognizing its justification, we cannot sufficiently emphasize the necessity of keeping it altogether out of the field of theoretic research, not because it is "philosophy," but because it is normative. When theoretic investigation of activities is combined with

their normative standardization, there is always the danger that the former will be subordinated to the latter with results detrimental to scientific validity. The method of normative standardization is essentially deductive: having established a supreme standard in any field—be it ethics, politics, religion, or aesthetics—the thinker deduces from it a systematic hierarchy of ideals and norms. He may use induction as an auxiliary method, either before establishing his supreme standard, in order to find what standards human agents actually follow in their activities, or after having established it, in order to determine the conditions under which they would accept and put into action his normative system; but in either case his selection and organization of data is apt to be conditioned by his normative purposes rather than by the theoretic criteria of strictly objective, inductive research.[1] Thus the history of philosophy (particularly, though not exclusively, between the third century B. C. and the sixteenth century A. D.) shows how difficult it is to comply with these criteria, if normative interests are allowed to influence theoretic investigation.

This does not mean that a theoretic investigation carried on to gain auxiliary information for normative purposes never can be objective and scientifically valuable, nor yet that all studies of human activities carried on in the past and within the vast field known under the vague term of "philosophy" have been dominated by normative purposes. All we wish to em-

[1] Of course, the criteria of scientific research are themselves normative in the logical sense; but to use standardized theoretic methods in investigating activities is a very different matter from demanding that the activities investigated should comply with the investigator's ethical, political, or even logical standards.

phasize is the existence of an essential, irreducible difference between the theoretic investigation of activities and their normative standardization, as well as the desirability of keeping the former completely separated from the latter, lest it should interfere with its progress, as it has so often done in the past.

Having thus, let us hope, prevented any possible misunderstanding, we can now define more exactly the humanistic way of approaching human activities. It is very familiar to us all in our everyday life.

5. *Experiencing Activities*

When I wish to ascertain at first hand what a certain activity is, just as when I wish to obtain first-hand information about a certain object, I try to experience it. There is only one way of experiencing an object: it is to *observe* it personally. There is also only one way of experiencing an activity: it is to *perform* it personally. Practical men insist on this: they will tell you that you cannot fully realize what they are doing until you do it yourself. Scientists have come to recognize this: the modern student in ergology sees the need of learning how to practice himself the various techniques of lower civilizations, such as working in stone, shooting with a bow and arrow, etc.; the philologist does not believe his personal acquaintance with a language perfect unless he has learned to speak it or at least (with dead languages) to write it; the student of religion tries to obtain first-hand experiences by sharing actively in religious ceremonies; the epistemologist and the methodologist realize that first-hand acquaintance with scientific methods demands active participation in scientific research. And so on. Actual performance is the *primary* source of empirical knowledge about activity.

This statement must be explained and developed in view of certain artificial difficulties which have been raised by philosophers in connection with the traditional dualism of "mind" and "matter." A philosopher or psychologist faithful to the old idea of an individual consciousness, within which perceptions, representations, volitions, feelings, emotions, are somehow enclosed or imbedded, interprets activity as a mental, "internal" fact. From this point of view, the only activity I could experience would be an activity belonging to my subjective consciousness. Experiencing an activity performed by anybody else would be impossible. As a source of scientific knowledge about human activities in general, actual performance would be worthless; all I could do with its help would be to make a theory of my own particular activities.

However, similar objections have been raised by philosophers concerning the possibility of experiencing the same objects as somebody else experiences. According to subjective idealism, the objects I experience are only my own "ideas" or "states of consciousness." I have no certainty that you and I are experiencing the same object or even that you actually exist and are not merely my idea or my state of consciousness.[1] But positive science has gone ahead without heeding such problems about objects; nor can it be stopped now by similar speculations in its study of activities. It partly waives them aside, partly reduces them to mere methodological difficulties which can be overcome from case to case.

The scientific assumption with regard to objects is

[1] This view with various modifications has been explicitly formulated by several German philosophers in the last quarter of the nineteenth century, such as Schuppe, Schubert-Soldern, Cornelius.

that every real object either is accessible to the experience of any person at any moment, if he puts himself into the proper conditions for experiencing it; or else can be logically inferred from objects which are empirically accessible to every person. Of course there is always the chance of mistaking experiences of separate realities for experiences of the same reality, and *vice versa*. The test for clearing up all such misunderstandings is furnished by the principle of closed systems. My experience of a tree or a religious myth may differ from yours, but this is scientifically irrelevant for the identification of this object; the tree or the myth is sure to be the same if it takes the same place as an element in a system—a forest or a religion—which we both share. Or on the contrary, my experiences of a tree or a myth may be similar to yours and yet our objects be distinct, as we shall find when we discover that your tree is located in a different part of our forest, or that your myth belongs to a different religious system than mine. This obviously presupposes that we can identify these systems, either directly by sharing them, or again by referring them to some wider system which we are sure of sharing.

Can a parallel principle be applied to activities? According to dualistic psychology, this would be impossible. For the reason why the same object can be experienced by you and me is that it exists in the "outside world" whether we experience it or not: its real existence is not limited to the actual fact of its being perceived or represented here and now. Whereas an activity exists only while it is actual; in this respect it is not like an object, but like a perception or representation of an object; it is only there while it is being performed.[1]

[1] This is the pivotal idea of the whole psychology and philosophy

Consequently, not only is it impossible for me to experience your activity, but I cannot even experience today my own activity of yesterday.

For scientific purposes, however, this view has proved entirely inadequate. It has produced an unbridgeable gap between the psychology of individual consciousness and the sciences of the cultural world, because of which this psychology has been of very little use in humanistic studies.[1] Of course, it is a truism that an activity does not actually exist for the agent except while he performs it; no more does an object exist for him except while he is observing it. But just as he is aware from the content and meaning of this object that it exists as an element of some system within which it can be observed by many people, so he is aware while acting that his activity manifests itself objectively, is occurring within some system, and that other people can also act within the same system just as he does: his activity is not altogether and exclusively his own, even though he performs it. We can express this briefly by saying that every activity has an objective *form* and an objective *function.* Its form is the way in which it deals with objects—"pattern" is nowadays the more popular term; its function is the share it has in constructing, maintaining or changing a system.

There is nothing metaphysical about these concepts; they correspond to familiar, everyday data of practical life. The assumption that activities have objective forms or patterns is tested every time somebody teaches somebody else how to perform an activity—drive a nail,

of Wilhelm Wundt; in a modified form it underlies also the psychology of William James.

[1] Wundt tried in vain to bridge this gap with the help of the concepts of "collective will" and "folk-psyche."

spin or weave, play golf, say prayers, write words, analyze a Latin sentence, compose a story. Teaching and learning, consequently all cultural tradition, would be impossible if activities were not "communicable"; and what is communicable is obviously not the subject's present act as such, but the way objects are actively handled. Nor could cultural systems be constructed by the cooperation of several individuals and maintained by a succession of them if the function of an activity in a system were not essentially independent of the individual subject who performs it, though different individuals may perform it somewhat differently. When a workman in a factory finishes off the unfinished part of an automobile passed to him by another workman; when a scientist continues an investigation which another has dropped; when a newly appointed official steps into the place of his predecessor: the function is one and continuous, though the functioning agents change. It does not matter for the production of the automobile, for the advance of science, for the maintenance of the office who does the work, provided it is done as it ought to be done.

Thus, when you and I perform "the same" activity, it does not mean that our two "subjective acts" are one, any more than experiencing the same object means that our "subjective perceptions" are one. It may mean one of three things:

a) That our activities are formally the same, though functionally distinct: this is called *repetition*. Thus, the pupil in school repeats the teacher's activity of writing a word or analyzing a sentence; the golf amateur repeats the stroke of the professional; the ergologist repeats the savage's performance with a bow. Functionally, these actions are distinct, inasmuch as for the

teacher writing the sentence is a part of his teaching function, for the pupil a part of his learning function; in golf, each man plays his own separate game; the ergologist shoots with bow and arrow as a matter of scientific experiment, while the savage does it in hunting or war.

b) That our activities are functionally united, but formally different: this is *participation*. For instance, the workers in a factory participate in the function of running it; the officials of a social group participate in the function of maintaining this group; the scientists interested in the same branch of science participate in the function of developing it. Formally, however, each worker, each official, each scientist may be doing something different, dealing with different objects in a different way.

c) That our activities are formally the same and also fulfil the same function: this may be called *reproduction*. People are reproducing over and over again the same technical patterns, going through the same ritualistic performances, playing the same musical compositions, reading the same books, teaching students the same geometrical demonstrations; though their personal purposes vary, there is a common objective function they all fulfil in such cases: it is that of perpetuating by reproduction the technical pattern, the rite, the musical composition, the contents of the book, the system of geometry.

Modern behaviorism has rejected the conception of a closed individual consciousness and concentrated on objective manifestations of activity. Unfortunately, in trying to get rid of the dualism of "mind and matter," it has simply adopted the naïve solution of age-old materialism: it preserves the "matter" side of it and

ignores the "mind" side, instead of going beyond both and searching for a new approach. Consequently, it treats as objective "behavior" only those activities whose form or pattern is manifest in handling material objects, and is inclined to assume that the function of every activity is mutual adaptation between the organism and its environment. Now, much of the content of cultural reality is non-material, even though symbolized by words or other signs, and even material cultural objects have meanings which are empirically observable, but not sensually given; obviously the patterns of activities dealing with this non-material aspect of culture cannot be deduced from those of activities handling material objects. Moreover, the functions of most cultural activities at higher levels of development have nothing to do with the individual's adaptation to his environment, but only with the preservation and development of systems which transcend and sometimes conflict with the needs of each individual participant.

A student of "behavior" who refuses to recognize any but sensual patterns and biological functions cuts himself off from innumerable first-hand experiences without which cultural life becomes incomprehensible. If really consistent, he cannot repeat, participate in or reproduce the activities of people who create and maintain religion, literature, science, philosophy, moral, political and economic organization, for those people themselves perform and experience their activities as if non-material objects and systems were as real as material ones—if not more so.

The whole question is not one of metaphysics or epistemology, but of scientific method. We may waive active experience altogether as source of knowledge, as John Watson does, and limit ourselves to outside ob-

servations of organic movements and biological experimentation. This means resigning the investigation of cultural activities altogether. But there is no reason whatever for doing it, since practical life shows that activities as experienced by the agent himself have an objective aspect which makes repetition, participation and reproduction possible. And this concerns all activities, not only handling objects of the material environment. A simple and obvious test is having several agents describe independently what they are doing when they repeat, participate in or reproduce an activity. If the meaning of the words they use is sufficiently stabilized, their descriptions will tally, just as the descriptions of a tree made independently by several observers. And this will hold true of eating, playing golf or producing an automobile as well as of reading a novel, canonizing a saint, discussing the philosophy of Kant, or demonstrating a mathematical theorem.

Thus, if we want to know what any particular activity or activity in general really is, we find that the humanistic approach is the only approach that brings us to the original source—the experience of the agent. And there is no difficulty about it, for we are all agents and each of us can experience the activities of others by repetition, participation or reproduction. But what does the agent experience when he performs an activity?

Psychologists have been trying to find this out for centuries. Many times introspection has discovered some specific experience which it believed could be considered "the experience of activity": the *actus purus* of the scholastics; Maine de Biran's consciousness of "effort"; Schopenhauer's "Will," Bergson's "*élan vital*" are familiar instances. Always rational analysis has dissolved the supposed specific experience of activity into a series

of emotionally tinted experiences of objective, chiefly organic, things and processes. And always, again, this analysis has been impeached by the claim—undoubtedly just—that, while activity is subjected to rational analysis, it is not being performed, and we can therefore no longer experience it. But the only alternative seems to be an appeal to some irrational power of experience, some kind of direct "intuition," simultaneous with and merged into activity itself—and this is the straight road to mysticism.

6. *Activity, Tendency, and Attitude*

Since the method of introspective psychology has thus far failed definitely to discover what the agent's experience of activity "really is," we had better waive it altogether. That is not the way for the agent to learn whether he is active or not, to find out what he is doing, or to distinguish one kind of activity from another. What is, in fact, given to him while he is acting is the same set of data (though with some differences of content and meaning) which is given to any observer of his behavior who knows the meaning of the objects this agent is dealing with: it is the dynamic objective manifestation of his activity, the gradual construction in actual empirical reality of a definite system of values—a poem or musical composition, a religious ceremony, a financial undertaking, an association. Activity is nothing but that which brings the construction of this system about: it is the primary factor of this construction. Only the construction itself is somewhat differently experienced by the agent than by the observer. There are two essentially distinctive characters of his active experience.

First, the system which is being constructed is somehow getting determined by the agent in advance, not in the sense of being "planned" or "foreseen" in its detail, but in that of being made to include some of the many possible values within the reach of the agent's experience and to have these values combined and modified in one of many possible ways. Writing a poem means selecting certain experiences to be expressed, organizing and modifying these experiences in mutual adaptation so as to produce an aesthetic unity, finding and combining symbols to express them in a way which will meet both the demands of this aesthetic organization and certain requirements of rhythmic cadence and rhyme. Organizing a store implies selecting certain economic needs of the community which it will aim to satisfy, finding some capital, renting or buying premises where the goods can be located, buying goods which are adapted to satisfying the needs, hiring and putting to work employees, selling the goods at a profit calculated in advance.

This prospective determination of the system by the agent is not experienced by him when his activity is going on uninterruptedly; he then experiences only the values as they are given to him, combined and modified by his activity. But if he begins his activity by thinking what he will do and how he will do it, or if at any later time during this activity he temporarily stops acting, the prospective determination of the system becomes a matter of actual experience. The term *defining a situation,*[1] or the equivalent terms of *defining* or *setting a practical problem* can be used to indicate this specific experience. The latter need not be accompanied by any rational reflection comparing and

[1] Thomas and Znaniecki, *The Polish Peasant*, p. 68.

weighing possible choices; rationalized situations are a special variety of situations in general.

Alongside of this prospective determination of the system, a retrospective determination goes on, a readaptation of the values already selected and of the modifications already made to fit whatever new values and new modifications are taking their place in the system. This retrospective determination is also not primarily experienced as such: only the particular values and modifications effected by it are given. But at any moment the agent can become aware of it, if he stops to connect a present situation with a past situation and realizes that he is solving or failing to solve the practical problem included in the latter.

These two distinctive characters of the agent's experience of the construction which his activity "brings about" will be best expressed by stating that an activity from the point of view of the agent's own experience of it is *a tendency to construct a system of values in the course of its realization.* The term "tendency" suggests both the fact that the system is prospectively determined, "intended," and that this prospective determination may or may not be fulfilled. At the same time, it is a sufficiently general term not to bear any specifically psychological implications, since it can be used also in biological and even in physical sciences. Indeed, a cultural tendency is fundamentally characterized, just as a natural tendency, by its objective manifestations in so far as these are determined in advance and not disturbed by other factors.

However, there is an important difference between cultural and natural tendencies, due to the fact that the cultural world is a world of values, not of things. Whereas natural tendencies are only manifested in

so far as they are being realized, a cultural tendency can manifest itself empirically not only in the course of its realization as activity, but also at other times as an *attitude;* and it does this when it only defines the situation without solving it.

We have seen that values have a positive or negative axiological significance when taken with reference to a cultural system, and that this significance depends on the bearing they have upon the system in connection with other values of the latter. While the system is being really constructed, this significance is empirically expressed in the very *act* of acceptance or rejection of the given values as its element; a successful or unsuccessful attempt is made by the agent to introduce the value into the system, or to prevent or counteract its interference with the system. Thus, during the performance of a public religious ceremony the positive axiological significance of an instrument of cult is expressed in the act of bringing and using this instrument at the proper moment; the negative significance of an "unbeliever" in the act of his expulsion from the sacred place.

When, however, as a matter of fact the system is not being constructed, this axiological significance of the values involved in its construction remains latent; but the original tendency, whenever it reappears without resulting in an active performance, shows itself in a specific attitude toward these values. Psychologically speaking, the attitude is a definite appreciation of a given object as desirable or undesirable; and this appreciation may range all the way from purely intellectual approval or disapproval to a most irrational emotion, and from a static "feeling" to a dynamic "wish." In objective terms, the attitude is a determination of

the active treatment the given value would receive in the system which tends to be constructed, if it ever really be constructed. The attitude is, thus, a potential substitute for the act.

For example, when an instrument of cult or an "unbeliever" is given in experience to an agent who is not at the moment participating in a religious ceremony, but still tends to do so under the proper conditions, the axiological significance of those objects will show itself in the fact that the agent will experience an attitude of reverence toward the sacred instrument, an attitude of scorn (perhaps combined with fear) toward the unbeliever.

A tendency is not, however, realized in a single act dealing with one value, but in a whole series of acts—the total activity of constructing a system of values. There may also be complex systems involving each a combination of tendencies subordinated to one dominant tendency. Consequently, a single attitude only partially expresses a tendency when the latter is not active, and the whole set of attitudes toward the various values involved in a system is needed to reconstruct fully a tendency. On the other hand, a particular value may be used in many different and disconnected cultural systems in all of which the agent participates at various times; his attitude toward this value may then be a complex substitute for many virtual acts, a number of various tendencies may be partially manifested in it. Thus, a believer's attitude toward an unbeliever may embody a variety of potential active consequences, not only religious but social, if he is a foreigner, or if he belongs to a different race or class.

Nevertheless, in every attitude some tendency is manifested, and every tendency while not active mani-

fests itself as an attitude, whenever any of the values it tends to utilize are given. In other words, values are not appreciated unless there is a potential tendency to use them in a cultural system; and there is no potential tendency to construct a cultural system, if the values belonging to this system are not appreciated. Instruments of cult collected and exposed in a museum are not the objects of an attitude of religious reverence even on the part of a person who would revere them, if he found them in a church ready to be used for worship; and if we see a man indifferent to the institutions of a social group, we know that he has no tendency to participate in the public life of this group.

In applying to empirical data this conception of the tendency, we meet two kinds of problems. First, why does a tendency manifest itself at one time as activity constructing a system of values, at another time merely as an attitude or set of attitudes toward one or several of these values? Secondly, why does a tendency when active succeed in some cases in realizing itself, solving the situation as defined and achieving the very system it started to construct, whereas in other cases it fails in its attempt at realization and its total result is different from the one intended? We must have definite guiding principles in dealing with such problems, otherwise we are apt to lose ourselves in the maze of concrete human life.

The first kind of problem refers approximately to the same facts as those covered by the traditional theories of "motivation," though viewed from a somewhat different standpoint. The leading principle of all theories of motivation is that human activity must and can be explained by human experience. The fact that a particular individual or collectivity begins to act in a certain

way at a particular moment is supposed to need explanation; and the explanation is sought in some particular experience which this individual (or collectivity) has had and which has "motivated" his activity. There are two types of theories of motivation: those which ascribe the supposed stimulating influence exercised by human experience to feeling, and look for the feelings aroused in the individual by the objects which he has experienced (i.e. perceived, represented, or imagined); and those which claim the priority of will over feeling and explain activity by the desires which the objects experienced provoke in the individual. When the concept of value was introduced into philosophy and psychology, this duality expressed itself in dual conceptions of values as objects of feeling (emotion, sentiment) and values as objects of volition (conation, desire).

But, however important the theory of motivation may be for the study of concrete human individuals (or, lately, concrete collectivities of individuals), it has no significance whatsoever for the investigation of cultural systems. We cannot and need not explain why a cultural system tends to be constructed at a particular moment by a particular agent. We cannot, because such an explanation would involve the entire past of the cultural world. We need not, because no science conscious of its task ever tries to solve this kind of problem. The main reason for using the concept of the closed system is precisely to avoid this kind of insoluble problem. A tendency is simply there and must be taken as given; and since a tendency is by its very definition a tendency to construct a cultural system, the activity constructing this system is its primary and original manifestation. Consequently, far from explaining ac-

tivity by the stimulation which the experience of a positive or negative value provides, we must derive the positiveness or negativeness of an attitude toward a value from the activity which uses or rejects this value in constructing its system. In other words, in cultural science, instead of asking (as psychology does) why X tends to perform a certain activity, we must ask why X, though tending to perform a certain activity, does not perform it but merely feels or wishes.

And this question is answerable in every particular case, provided only we have sufficient data and use a proper technique in analyzing them. It all depends on how the agent defines the situation. If some of the values viewed as essential to the system which tends to be constructed seem inaccessible at the time when the tendency appears, the situation is defined as *impracticable* and no active solution is attempted, but the tendency manifests itself in attitudes, positively appreciative toward such values as are in harmony with the system, negatively appreciative toward those which hinder its construction.

For example, when obstacles impossible to overcome prevent two people from marrying, their tendencies to marry manifest themselves in attitudes of romantic love toward each other and indignation at the obstacles. The attitude of reverence to religious values is never as distinctly experienced as when persecution forcibly represses activities of religious cult. When a scientist is prevented by any reason from doing active research, he develops attitudes of dogmatic certainty towards the knowledge he already possesses. And so on.

Of course, the definition of a situation as practicable or not may be "mistaken" from the objective point of view, or conditions may change. What seemed an in-

superable obstacle to the realization of the tendency may prove sometimes on closer observation no obstacle at all, or may be removed—in which case the situation becomes redefined as practicable, the potential tendency actualizes itself, attitudes pass into acts. Or, *vice versa*, after activity has started, unforeseen difficulties may be discovered or factors external to the situation thrust new obstacles in the way of the tendency—and then the situation is redefined as impracticable and acts pass into attitudes.[1] Oftener still, activity becomes interrupted for the time being only, with the expectation of being resumed again. Most of our protracted activities occur in this way: actual performance stops and the tendency passes into the potential stage, because our organism is too tired to continue its rôle as instrument, because some artificial instrument becomes spoiled, or because we begin to collaborate with some one else—in a word, because a situation has been reached which we define as temporarily impracticable. After a time, the organism gets rested or our collaborator finishes his task: the situation appears practicable again and the tendency passes from attitudes to acts.

Taking for granted that everything that can be said about tendencies refers to tendencies as they appear in actuality "here and now," wherever and whenever an agent "has" them, and that the question why a particular agent "has" a particular tendency at a particular moment does not concern us at all, we can formulate the following leading principle: *A cultural tendency is always active unless hindered by an internal practical obstacle.*

[1] As will be seen later on, situations impracticable for one tendency become reorganized and solved by some other active tendency; still, the original tendency remains as an attitude.

We shall refer to this principle as the *principle of spontaneity*, since it presupposes that activity needs no stimulus. An obstacle is "internal," if it has been included in the definition of the situation; and only such an obstacle prevents the tendency from being active. If it remains outside the situation as defined, that is, if the agent does not realize that because of it some essential elements of the system aimed at are inaccessible, the obstacle does not hinder the activity from going on, though it deflects its course and modifies its results.

Let us now consider our second problem. Assuming that a tendency is active does not at all mean that it will succeed in realizing itself by obtaining the results intended. Nor, on the other hand, is the fact that a tendency has been hindered from acting and manifests itself only in attitudes equivalent to a failure on its part to reach the results which constitute its realization. A situation defined as practicable may become actively solved indeed, but the solution prove entirely different from what was originally expected; whereas in defining a situation as impracticable and not trying to solve it actively, the tendency may be only biding its time and preparing for the expected solution whenever the obstacle be removed. Failure is not doing nothing, but doing something different from what was meant to be done.

Here again cultural science must take exactly the opposite stand to that which psychology has taken. For, while psychology tries to explain why the agent starts to act, and looks for explanation in emotionally or volitionally stimulating experiences of this agent, it waives entirely the problem of the objective results of his activity. Its interest is confined to the active process

as a psycho-physiological process. The older psychologist views this process as confined within the body and consciousness of the individual; the behaviorist looks upon it as a process of interaction between the organism and its environment; but for both the interest centers in the agent as a psycho-biological entity, and activity is defined in terms of this entity, not in terms of its results. Consequently, the problem of the results is from the psychologist's point of view theoretically unlimited and is implicitly regarded or even explicitly characterized as such. Given, indeed, activity as a psycho-biological process, the results of the activity must be defined as the total effect which this process as a cause produces in the "outside world" or in the environment of the agent. But this effect, so it seems, cannot be scientifically determined beyond the first stage of organic movement, for it combines with the effects of numerous other processes going on in the concrete reality in which the active individual lives, and these soon form an inextricable maze of facts mutually influencing one another.

For the cultural scientist, however, since he views activity not with reference to the agent, but with reference to the system which it tends to construct, the results are what matters, just as they are for the agent himself. He defines these results, of course, in the same way the agent does, in terms of the agent's values, with the humanistic coefficient, and not as natural processes; they are "products," not "effects." And having waived the psychological problem of the "motives" or "stimuli" which have made the agent act, we find the problem of results scientifically limited and capable of being solved.

Activity starts as a definite series of acts gradually constructing a certain system of values, and once we

have discovered the dominant tendency which manifests itself in these acts, once we have found the system that is being constructed by them, it is easy to define the results of this activity in terms of this system, to determine whether it has succeeded in constructing the particular system it intended to construct or not; and if a different one, how different. In the first eventuality, there is no problem whatever. In the second, there is a problem indeed, and we have to explain the failure of the activity to construct the system it started to construct. This we do by finding the factors which made it deviate from its original course. For example, we find a musician starting to play a certain sonata or a religious congregation beginning to perform a certain rite. Once we know what this sonata or this rite is for the respective agents, we can easily determine at the end of the musical or religious performance whether it has ultimately become what it was originally intended to become. If so, there is no problem: matters are as we expected them to be. Suppose, however, that the musician introduces variations into the original composition or the congregation abbreviates the performance by omitting certain sections of the ritual, we naturally ask why, and look for explanation in some perturbing influences.

In a word, from the standpoint of cultural science as a theory of cultural systems, what primarily matters about human activity that is not prevented by internal obstacles from going on, is whether its results are what they were intended to be; and if not, then why not. And here, to make scientific analysis possible, we assume as the second fundamental principle of cultural research that *a tendency once active always achieves the construction of the system of values it started to construct and*

no other, unless deflected by perturbing factors. We call this the *principle of achievement.*[1]

Of course, both the principle of spontaneity and the principle of achievement are heuristic assumptions which, like the fundamental principles of mechanics or of thermo-dynamics in physical science, cannot be directly proved or disproved by any particular fact of experience, but only tested by their continued application as instruments for the scientific interpretation and explanation of empirical facts.

7. *The Duration and Extension of Cultural Systems*

The construction of a system of values is a real dynamic occurrence which involves a definite series of facts and goes on in empirical cultural reality; it brings modifications into certain existing objects and their connections, and produces some object or connection that did not exist before as an empirical datum, at least within the sphere of experience of the agent, even if it did exist in the real world at large. When a poet recites his poem, the words and phrases of this poem with their particular meanings surge up in his memory out of the mass of words and phrases in his language; a complicated series of movements of his lips, tongue, throat and lungs goes on, through the instrumentality of which these words and phrases become expressed

[1] I first used this principle in the study of social actions in the *Laws of Social Psychology*, Poznan-Chicago, 1925, p. 64. It was then formulated as follows: "A social action, once begun, continues to its purposed end, unless there are factors interfering with its continuation." I have since then come to the conclusion, however, that this principle is applicable to all cultural systems and that, consequently, it should be formulated somewhat differently, as here in the text.

aloud in a definite order, with a definite intonation; and the total result is a specific aesthetic experience given to himself and his hearers. A religious group constructs collectively, under the leadership of a priest, a religious ceremony as a system of sacred objects and mythical entities, handling these objects and influencing these entities in such a way as to produce a sanctification of all the participants; and though to the unbelieving observer some elements, connections and processes in this system are unreal, we must remember the humanistic coefficient and admit their reality in the religious sphere as a part of the cultural world, though not in nature. The managers, technicians, foremen and workmen in a factory, by performing innumerable bodily movements and keeping the machines of the factory running, are able to take raw material, modify it, divide it, combine it, and after a series of different partial changes, to bring out the finished product—safety pin or automobile. This series of actions they repeat indefinitely.

The whole existence of a cultural system as a system of values is essentially founded on those series of actions by means of which the system is being actively constructed. It is a "kinetic" existence, and in this respect the cultural system has a certain similarity to those natural systems which are systems of processes rather than of things. But there are important differences, obviously due to the humanistic coefficient of cultural systems. Duration in time and extension in space do not mean the same for cultural as for natural reality.[1]

A kinetic natural system is supposed to exist unin-

[1] Cf. the author's *Cultural Reality*, University of Chicago Press, 1919.

terruptedly in a continuous time. But outside of the simplest varieties of cultural actions few cultural systems have such an uninterrupted duration, and those which do are either very shortlived, or else are intentionally made to function without interruptions by means of a complicated social arrangement in which individual or group agents take periodical turns in performing the same activities, and these performances partly overlap. The first case may be exemplified by a mob as a social group; the second by certain mechanical factories with their changing shifts of workers and overlapping technical processes. This second instance shows that such a continuity is a minor matter for the duration of a cultural system. Some factories are run continuously, others stop once a week for thirty-six hours, others stop every night: all depends on economic considerations. When a factory is working continuously, it is usually because the organization of human activities is being adapted for economic purposes to the continuous functioning of some physical system artificially made (e.g. the thermodynamic system of a steam engine).

Now, the factory which stops every night; the association which meets at rare intervals, functioning at other times only vicariously through the persons of its officials, representatives and members, or not at all when neither officials nor members act for it; the rite which is performed at stated periods, but not otherwise; the poem which is recited or the musical composition which is played from time to time: all these do not disappear altogether from the world in those intervals during which they are not actually functioning. They preserve a kind of latent existence, a potential reality. Their elements—the values which compose them—endure in their content and meaning, and can be ex-

perienced indefinitely. There are the buildings, materials and machines of the factory, sensual data whose meaning *as* buildings, materials and machines is obvious to anybody prepared to understand their use. The association has an office, symbols like a seal, a name (printed or written), a flag; the individuals who are its members live and preserve in their own experience and that of outsiders this characteristic of membership, this specific social meaning which designates them as belonging to the association. And the attitudes towards these values in which the tendency of the system manifests itself while inactive still have a real influence on human activities that shows itself whenever these values are to be used: they oppose a power of inertia to any use which conflicts with the axiological significance these values have acquired in their system. Thus, cases of so-called sabotage, i.e. attempts during a strike to spoil the machines used in a factory, though easy and effective, are relatively rare, not so much because of economic or moral considerations as because they provoke emotional opposition on the part of those skilled workers who consciously and planfully use these machines in normal times. An attitude of reverence would prevent a soldier from using the flag of his regiment for private purposes even when it is not being publicly used by the regiment as a group.

Unless, however, a cultural system is from time to time being actually made to function and brought into explicit dynamic reality, it loses gradually this latent existence which we are describing. Even if the values which enter into its composition should endure, the connection between them is loosened, and their axiological significance dwindles away. An association which does not meet and whose functionaries seldom act in

its name dissolves gradually, often imperceptibly; a deserted farm or mine after a while ceases to be a farm or a mine for any purpose; a custom which has ceased to be periodically acted upon is no longer a custom and becomes forgotten; a language which is no longer used actively by anybody is not only what is commonly called a "dead language" (i.e. one which, like ancient Greek, does not exist in speech, only in writing for those who can read it, and consequently does not evolve) but a non-existing language. In short, a cultural system must be maintained in existence by human activity.

As long as a cultural system is maintained in existence, it is still in the course of its development; its construction has not been finally achieved; every case of its actualization contributes something to its composition along the lines of its structure. This may be rather difficult to understand, unless we realize that there are cultural systems which have a definite term of duration, are meant to be "finished" after a certain result has been reached, whereas others are meant to last indefinitely, unless prevented by external obstacles, and every result actually achieved leaves still an opening and a demand for further results. All actions, simple or complex, belong to the first type: writing a letter, buying a house, building a railroad, fighting a war, are dynamic systems of values organized by an activity which proceeds to a final realization, an "end." The duration of other kinds of system may also be limited in advance by being subordinated to an ultimate result. Thus, an educational relation is intended to be finished when the educand's person has been formed in accordance with a certain model. In short, groups are constituted for the realization of explicit purposes and are not meant to last after such purposes have been

achieved, whereas a social custom, a moral ideal, a scientific theory, a style of art, a religion, a corporation or a state are systems with an indefinite duration. They are being kept in existence by people performing—continuously, periodically or sporadically—activities in accordance with certain rules or leading principles, and thus introducing again and again new values into the system. The new values either replace other values which have been eliminated or dropped out, as in a factory where finished products are sent to the market and new products have to be made, in a traditionalistic religion where the faithful and their belongings continually fall out of the state of sanctity and have to be sanctified over and over; or else they are super-added to existing values, as new works of art manifesting a certain style or new data to which a scientific theory is applied. These activities may be reproductions of other activities or original functional extensions of the leading principles. But these are later problems.

The question of the existence of cultural systems "in space"—or, rather, their "extension"—though familiar to the ethnologist, historian and geographer, is seldom fully grasped in its essential implications. We speak of the "area" of a custom or the "reach" of a language; we say that myths, inventions or literary products "spread" more or less widely. Are these merely figures of speech, or do they indicate a real objective character of the system? What we obviously mean by such expressions is that a custom, a religion, an invention is practiced by the people, or some of the people, inhabiting a certain geographical territory; the traveller who reaches this territory and gets into contact with these people can observe it directly and personally. But the question is whether in discovering and expres-

sing this fact we discover and express only something concerning these people or something concerning the custom, religion or invention as well. Does it make a difference to the cultural system itself how widely it "extends" in this sense? For we must agree that it does not make any difference to a natural thing—say, to the moon—how many people worship it or make love by its light. Here again the fundamental difference between natural and cultural data comes in.

Since the cultural system is what it is because of human experience and since the basis of its reality is its actual construction, the fact that it may be simultaneously constructed by many human agents must have a bearing on its objective existence in the cultural world as much as the fact that it may be successively constructed time after time. The trouble is that, although we can easily think of it as lasting in time through a series of actualizations, we cannot properly apply ideas of space to its simultaneous actualizations: we cannot say that a myth or a custom is located in space and occupies space as material objects and systems do. I have tried to introduce for this reason the conception of non-spacial cultural extension.[1] Assuming every human individual to be a distinct center of experience and activity, as he is, the totality of such centers at any time constitutes, so to speak, the living frame of concrete cultural extension within which all culture exists. Every cultural system extends over a part of this frame corresponding to the number of individual agents who participate in maintaining it. Since, however, the individual is a cultural center in this sense [2] only

[1] Cf. *Cultural Reality*, Chap. III.

[2] I believe that the term "cultural center" is more strictly applicable to an individual than to a community, though it is usually

in so far as he is a conscious agent and not in so far as he is an organic body, human ecology is of little assistance in determining the extension of cultural systems. What matters for problems concerning this extension is not how individual bodies are located in geometrical space, but how individual agents are able and willing to communicate and cooperate with one another. Spacial distance seems to be, indeed, a conditioning factor in cultural expansion, but even this factor must be taken not absolutely, but in relation to the technique of transportation [1] and of oral and written communication. And there are innumerable other factors facilitating or hindering the extension of any given system: identity or difference of language, social solidarity or social opposition, intellectual interest, religious taboo.

But when a system is thus maintained in existence only by multiple activities, contemporary or successive, each of which introduces into it some new values at least and often also new and original forms, what are the criteria which permit us to judge of the identity of a system in duration and extension? How can we tell whether a particular piece of music, mythical story, religious ceremony or scientific conception actualized by Agent A at the moment t^1 is, or is not, the same as the one Agent B actualizes at the moment t^2? How can we be sure whether a state or a religion found at a historical period p^1 is the same, or not the same, as the state or the religion which existed at a preceding

employed with regard to the latter by ethnologists and historians; for, exactly speaking, a community from which culture radiates is a multiplicity of more or less productive individuals who are cooperating in some of the activities.

[1] Prof. Erle F. Young is working on an ecological scheme which would substitute distance measured in units of time used for transportation instead of distance measured in units of mere length.

period p, particularly if several centuries elapsed between? How do we know whether two works of art do, or do not, belong to the same style, whether two scientific studies are, or are not, parts of the same theory, whether two dialects are both variations of one language or different languages?

The common view is that the identity or non-identity of two systems is determinable by outside observation on the ground of their similarity or dissimilarity. In such fields as art and literature a sharp distinction is drawn between "creating" a new and original system and merely "copying" a system that already existed; and the special term of "plagiarism" has been invented to indicate a work that pretends to be original when it is a mere copy—judged to be so by its similarity to the original. In the history of science, religion, politics, it is also a usual procedure to judge whether a theory, a religion, a state has, or has not, remained the same through the changes it has undergone, judging by the importance of these changes. In identifying a word, a tale, a technique, a custom in different countries, the ethnologist or "cultural anthropologist" is chiefly influenced by the similarity of the word as pronounced, the tale as told, the custom as observed in those countries.

Obviously, however, similarity is not a sufficient test of identity. From the point of view of a generalizing and objective cultural science the inherent difference between a system "created" and a system merely produced" is one of degree, not of natu
most original innovation can be viewe
of something that existed before;
evolution is unusually rapid and
and bounds, there is still a certain cq

sense that every new system appears in consequence of a modification, differentiation or integration of some system or systems that are already in being. On the other hand, at every repeated construction a particular system is really somewhat different from what it was at any preceding construction, for it is being constructed in somewhat new circumstances; and whatever efforts may be made to maintain it such as it was by adapting the circumstances to it, some reciprocal adaptation of it to the circumstances is indispensable. Such adaptations may be very slight; and if there is no definite trend in the changes of the circumstances, they may be ignored. Thus, a religious rite, a social custom, a process of technical production is in effect slightly different at every actualization; but their variations are not significant as long as they do not affect subsequent actualizations.

But sometimes such slight variations agglomerate, and after a time we find that the rite, the custom and the technical process have changed as deeply as if they had been suddenly modified by some important innovation. Is it still the same rite or custom, or a new one? And if the latter, when did the old one disappear and the new one emerge? The criterion of similarity is even more difficult to apply when it comes to determining the identity of systems which coexist in extension. Take two factories: their composition and structure may be so similar as to be almost indistinguishable at first glance, and yet they are obviously separate systems, though belonging to one class. But the technical method which was invented and patented years ago and now makes possible the functioning of these factories is "the same" identical system in both cases, not two similar systems, as is best proved by the fact that it is

protected by one patent only. A marriage ceremony as used in a certain country is manifestly one identical system in all its actualizations; but every one of the conjugal relations that start with the performance of this ceremony is a separate relation. Why is this so?

The only key to the solution of all these difficulties is the humanistic coefficient. The construction of a cultural system is mere reproduction of a system already in existence when it is intended to be such by the agent and taken to be such by other participants, even if the copy is very different from the original; it is the creation of a new system when it is intended to be such and taken as such, even if the new system is exactly similar to another already existing. A musical performance which is meant to reproduce a certain composition is still the actualization of this composition, however dissimilar from the composer's performance; whereas a plagiarism is a separate work, however similar to the plagiarized original. A rite, a custom, even a personal "habit" remains identical as long as the agent intends to uphold it as the same, though it may change greatly in composition and structure; whereas at other times even a slight deviation from a custom may constitute a break of the custom, if it is intended to break it and judged to be a break by the community. A state after a revolution is still the same state, however changed, if its authorities and citizens treat it as such; it is not the same if they repudiate its identity—compare Germany and Russia. The marriage ceremony is the same at all weddings, because the married couples and their families want to have the same traditional ceremony sanctioned by the church or some other group; whereas the conjugal relation is a different system in each case, because each married couple considers itself

unique. Similarly, whether a work of art does or does not belong to a certain style, whether a monograph is or is not part of a scientific theory, can be decided only when we know what was the intention of the artist or the scientist in this respect at the time he was producing that particular system, and how the latter is regarded by other artists, connoisseurs, scientists or critics.

The humanistic coefficient is not an infallible criterion, nor is it always easy to apply. There are often conflicts between the intentions and experiences of different agents cooperating in a cultural system: thus, an artist may view his work as belonging to a certain style, whereas his critics may deny it, or *vice versa*. Often also the agent, individual or group, may be mistaken about his own intentions when reflecting about them. But these are problems of scientific technique to which we shall return later on, and they do not affect the fundamental principles expressed in the humanistic coefficient.

References

The distinction between nature and culture is old: already the Greek Sophists opposed that which was rooted in the "nature" of things to the products of human "convention." But until modern times this distinction was obscured by various axiological and metaphysical considerations. To the Greeks and Romans "nature" meant much more than the world of material objects and processes, organic and inorganic. It was conceived to be, either by itself or through divine agencies, the foundation of any cultural order which was taken as absolutely valid: the logical order of knowledge, the moral order of conduct, the æsthetic order of art, the legal order of the state, even

the grammatical order of language. Traces of this still remain in such intellectual survivals as the ideas of a "natural law," "unnatural behavior, i.e. behavior conflicting with supposedly universal standards, "natural crime," i.e. crime which offends against the necessary conditions of group life (Cf. Garofalo, *Criminology*).

Later on, chiefly in Christian speculation, a cleavage was made, not between nature and culture, but between the natural and the divine order. Human culture was regarded as a combination of natural and divine elements. Whatever in it was true, good, beautiful, well-ordered, etc. came from God; what was undesirable and imperfect was due to nature, or if manifestly evil, was often derived from Satan. This division has left a marked influence in modern thought: thus, the philosophy of Kant, and even more so that of Fichte, assumes a fundamental dualism in man himself, due to his participation in two distinct orders—the inferior order of Nature and the superior order of reason. Culture is a combination of both; in some of its products one, in some the other order predominates. Cf. Kant's *Kritik der Praktischen Vernunft* and *Kritik der Urtheilskraft;* Fichte's *Die Bestimmung des Menschen.* This dualism persists in some of the modern German theories of cultural phenomena. See, for instance, Münsterberg, *Philosophie der Werte,* Stammler, *Die Lehre vom richtigen Rechte,* Kelsen, *Der soziologische und der juristiche Staatsbegriff.*

The growth of geographic and anthropological discoveries gave a new character to the distinction between nature and culture. Peoples in low stages of civilization were at first regarded as "savages" living in a "state of nature" in contrast to "civilized" or "cultured" peoples: Rousseau's exaltation of savages as unspoiled representatives of the natural condition of man and his criticism

of civilization is a well-known instance. And this attitude also persists in more recent times. Thus, the central problem of Vierkandt's book *Naturvölker und Kulturvölker* (Leipzig, 1896) concerns the standards of this distinction, though it no longer appears in its naïve early form.

The full realization of the distinctive character of all culture as against nature has been brought about by the progress of historical and ethnological research. The various historians of "culture" or of "civilization" (the two concepts as they generally appear in literature are interchangeable, though there are justified attempts to separate them) have been forced to look for a common definition of all cultural phenomena, since they included them all under the same denomination, and usually the problem of the relationship between nature and culture thrusts itself upon their attention. Leaving aside for the time being the theories of culture involved in such philosophic systems as those of Hegel, Marx, Comte and Spencer, we refer here to the following historical works:

Buckle, H. I., *The History of Civilization in England*, London, 1872; New York, 1892.

v. Hellwald, *Kulturgeschichte in ihrer natürlichen Entwicklung*, 3rd ed., Stuttgart, 1884.

Lippert, J., *Kulturgeschichte der Menschheit* (translated by G. P. Murdock as *The Evolution of Culture*, New York, 1931).

Ratzel, Fr., *The History of Mankind*, translation of A. J. Butler, London, 1896-98.

Gothein, E., *Die Aufgaben der Kulturgeschichte*, Leipzig, 1889.

Grupp, E., *System und Geschichte der Kultur*, Leipzig, 1892.

For ethnology and anthropology Tylor seems to have definitely established the term *Primitive Culture* by his book under that title. Though anthropologists are often inclined to take the concept for granted, still there have been important analyses and discussions of specific problems which throw a light on the problem of culture in general. The following works may be mentioned:

Lowie, Robert, *Culture and Ethnology,* New York, 1917.
Goldenweiser, A. A., *Early Civilization,* New York, 1922.
Kroeber, A. L., *Anthropology,* New York, 1923.
Winter, C., *Man and Culture,* New York, 1923.
Dixon, R. B., *The Building of Cultures,* New York, 1928.
Malinowski, B., Article on "Culture," in *Encyclopedia Britannica.*

The majority of students of culture are inclined to treat it as essentially dependent upon natural factors. Of these, race and the geographic environment have played a predominant part in general theories of culture. The racial point of view has been most radically expressed by Gobineau, *Essai sur l'inégalité des races humaines,* Paris 1853, which gave birth to the famous conception of the superiority of the Nordic race. Hellwald in the work mentioned above emphasizes the racial factor. So does Gumplowicz, *Der Rassenkampf,* Innsbruch 1883; Vacher de Lapouge, *Race et milieu social,* Paris 1908; Hankins, F. H., *The Racial Basis of Civilization,* New York 1926.

The influence of the environment dominates in the well-known theory of Ratzel, *Anthropogeographie,* Stuttgart 1891, popularized by Semple, E., *Influences of Geographic Environment,* New York 1911. See for detailed discussion of both the environmental and the racial schools Sorokin, *Contemporary Sociological Theories,* Chapters III and IV.

The most comprehensive general theory of culture, that

of Wilhelm Wundt, *Völkerpsychologie* (11 volumes, Leipzig, 1900-1912), is almost free from naturalistic preconceptions. Dilthey in his *Einleitung in die Geisteswissenschaften* (I, 1883) was one of the first to demand a complete independence of cultural knowledge, though this independence was already implied in Hegel's great attempt at a philosophical synthesis of culture as the domain of "objective Spirit."

Recently, under the influence of the present cultural crisis, the problem of culture is being very widely discussed, usually with practical ideals in view. Among the many books which have recently appeared, the following may be mentioned:

Beard, Charles A. (editor), *Whither Mankind*, New York, 1928.

——— *Toward Civilization*, New York, 1930.

Stoddard, L., *The Revolt Against Civilization*, New York, 1922.

Storck, John, *Man and Civilization*, New York, 1927.

Rugg, Harold, *Culture and Education in America*, New York, 1931.

Counts, George S., *The American Road to Culture*, New York, 1930.

——— *Education in Industrial America* (in print).

I do not know who—after the economists—was the first to use the term "value" to denote cultural phenomena in general. Personally, I took it from the works of Frederic Nietzsche. However, as I tried to show in the work on *The Problem of Values* referred to in the text, as long as "values" are not clearly distinguished from natural "things" as a separate category of objective data, and the view prevails that things become values only by having subjective "volitions" or "feelings" attached to them, the

concept is of little use for a positive science of culture. For instead of investigating objective relations between values, we are forced then to investigate the subjective psychological process of "valuation," which is all right for the psychologist or philosopher, but not for the student of language, art, religion, economics, etc., where systems of values are given as objectively existing, and human volitions and feelings have to be understood with reference to them. By defining the value as an object which possesses a meaning, objectively and independently of any psychological subject, and which is therefore distinct from the natural thing that as such has no meaning, we are simply expressing the essential character values possess in human experience. They are appreciated because they are meaningful in themselves, not *vice versa;* their axiological significance is dependent on their relation to other values in the course of an activity, not on their relation to the psychological subject.

The most important works on values from the psychological point of view are:

v. Ehrenfels, Ch., *System der Werttheorie,* Leipzig, 1897-8.

Urban, W. M., *Valuation, Its Nature and Laws,* London, 1909.

For the sociologist it has been easier than for other students to detach the value from the psychological process of valuation, for the axiological significance of values for a community obviously cannot be reduced to psychological processes of individuals. Thus, the approach to values contained, for instance, in Bouglé's book *Leçons de sociologie sur l'évolution des valeurs* (translated by H. S. Sellars under the title *The Evolution of Values,* New York, 1926) is rather akin to ours.

Activity has been an object-matter of investigation since Aristotle, whose theory (as expressed chiefly in his *Poetics*, *Metaphysics* and *Ethics*) is probably the most important single contribution to the problem. Much was written about activity in mediæval philosophy, but usually with reference to moral and religious standards. In modern times, unfortunately, there has been a separation of the psychological from the objective aspect of activity. The former is connected with the whole problem of will, while studies of the latter are subdivided into as many independent sections as there are branches of cultural research and scattered in innumerable works. It would be possible to glean a rich harvest by drawing upon monographic and systematic works in ethics, politics, æsthetics, economics, technology, theory of science, for studies concerning these various kinds of cultural activity viewed in their objective manifestations; but comprehensive synthetic studies are practically all made from the psychological point of view. Some of them are contained in general works of psychology, sociology or ethics; others deal particularly with this problem. In mentioning here a few, I cannot claim to give the reader an adequate bibliography on the subject, but simply to indicate some of the works which have influenced positively or negatively the views expressed in this chapter.

Fouillée, Alfred, *La psychologie des idées-forces* (Alcan).
Lapie, P., *Logique de la volonté* (Alcan).
Paulhan, F., *L'activité mentale* (Flammarion).
Pareto, V., *Traité de sociologie générale*, Paris-Lausanne, 1917-19.
Pradines, M., *Critique des conditions de l'action* (Alcan).
James, W., *Principles of Psychology*, New York, 1899.
Woodworth, R. S., *Dynamic Psychology*, New York, 1918.

Thorndike, E., *Educational Psychology,* New York, 1913-17.
Dewey, John, *Human Nature and Conduct,* New York, 1922.
Watson, J. B., *Behaviorism,* New York, 1924.
Kantor, R. J., *Principles of Psychology,* New York, 1924-26—the most thorough and consistent theory of activity from the point of view of the stimulus-response scheme, which is entirely rejected in the present work.
Ward, L., *Psychic Factors of Civilization,* New York, 1906.
Dewey and Tufts, *Ethics,* New York, 1913.
Münsterberg, H., *Die Willenshandlung,* Freiburg, 1888.
——— *Philosophie der Werte,* Leipzig, 1908.
Wundt, W., *Ethik,* Stuttgart, 1903.

It is hardly necessary to give here references to the various discussions of "instincts," "wishes," "volitions," "impulses," "drives," and other psychological conceptions of active tendencies. The works of social psychology listed among the references to Chapter III contain much on this subject; meanwhile, we can refer to the bibliography contained in Sorokin, op. cit., Chapters XI and XII, and also to Eubank, E. E., *The Concepts of Sociology* (Heath & Co., 1931), Chapter X on "Societary Energy." For the concept of "attitudes," see the papers and bibliographies of E. Faris, R. E. Park and L. L. Bernard in the book *Social Attitudes* edited by K. Young in honor of W. I. Thomas (New York, 1931). Concerning the literature on this problem we have only one remark to make. However an active tendency may be called—volition, impulse, drive, wish, etc.—it is not the name that matters, but the concept which it symbolizes. And we find a radical difference in this literature between two kinds of concepts: in one kind

of concept the tendency, wish, drive, impulse, etc. is defined exclusively by its objective manifestations in the world of values, that is, as the empirically given facts of constructing, maintaining or changing cultural systems. (In the very same way is physical energy defined exclusively by those empirical facts which are regarded as its manifestations in the world of material data.) In the other kind of concept, the volition, wish, drive, desire, impulse is viewed as a psychological reality, a process going on within the mind or body of the agent and taken in connection with other processes within the same mind or body.

The first concept is used methodologically in the same way as the physical concept of energy: it serves as an instrument of analysis in *describing* the becoming of cultural systems. The second is used as in mediæval philosophy the concept of virtue or force was used: it serves to *explain* cultural phenomena in terms of an effective cause (*causa efficiens*) external to these phenomena themselves and rooted in the spirit or body of man. The danger of the term "instinct" lies in the fact that it suggests the latter use. Sorokin in criticising "psychological sociology" objects that such concepts as "desires," "interests," "wishes," "attitudes," add nothing to the description of concrete facts. The objection is based on a misunderstanding. If these concepts are used scientifically, they are not meant to add anything to what we already know about concrete facts; their purpose is to *organize* our knowledge about facts rationally, to help us compare, analyze, classify, synthesize facts. If they pretend to add to our knowledge of facts a knowledge of what brought these facts into existence, their use is not scientific, but metaphysical, and Thomas and I were among the first to protest against it.

Concerning the duration and extension of cultural systems we refer again to the works of Goldenweiser,

Kroeber, Wissler, Dixon mentioned above. To these may be added:

Vierkandt, A., *Die Stetigkeit im Kulturwandel,* Leipzig, 1908.

Graebner, F., *Die Methode der Ethnologie,* Heidelberg, 1911.

Wissler, C., *The Relation of Nature to Man in Aboriginal America,* New York, 1926.

Kroeber, "The Culture-Area and Age-Area Concepts of Clark Wissler," *Methods in Social Science,* pp. 248-265.

Wallis, Wilson D., *Culture and Progress,* New York, 1930.

Ellwood, Charles A., *Cultural Evolution,* New York, 1927.

Smith, Malinowski, Spinden, Goldenweiser, *Culture: The Diffusion Controversy,* New York, 1927.

CHAPTER III

The Data of Sociology

1. *Sociology as Theory of "Societies" or of "Communities"*

Sociology, from the time it consciously began to be constituted as a separate theoretic discipline, distinct from political science, took a peculiar position with regard to the choice and determination of its object-matter, which persisted up to the end of the nineteenth century and partly survives even yet among sociologists of a naturalistic bent. While the particular data in which it was primarily interested belong to the domain of culture, it conceived them as components of natural systems. This conception, already outlined in the seventeenth century, became fully developed in the middle of the nineteenth with Comte, Spencer and their followers. Sociology was meant to study "societies." A "society" was conceived as essentially a natural closed system of bio-psychological human individuals. There were other more or less similar systems in nature: aggregations of unicellular organisms, multicellular organisms with differentiated organs and functions, associations of multicellular organisms—homogeneous, like herds and flocks of animals, and heterogeneous, like plant communities. As every sociologist knows, the seeming analogy indicated by Comte between a "society" and a multicellular differentiated organism has

been widely exploited by Spencer, Schäffle, Lilienfeld, Worms, Novicov, and many others.

Three principles were used, in various proportions and combinations, to circumscribe this natural system. First, a "society" had to occupy more or less exclusively a geographical territory; secondly, it was expected to possess a certain degree of racial homogeneity. These two were purely naturalistic principles: geographic isolation and racial composition of a human "society" could be ascertained by the same methods of external observation as the isolation and composition of a plant or a colony of polyps.

But the third principle made a breach in the consistency of the naturalistic standpoint through which an enormous mass of cultural data was introduced into this system. A collectivity of human beings of a certain racial stock (pure or mixed) inhabiting a certain territory constituted a "society" only if they belonged as members to a social group—horde, family, tribe, gens, village, city, state—or at least to a conglomeration of interconnected groups. Now, whatever might be said of "animal societies," human groups are cultural products; membership in a group, and even the mere existence of a social group, however rudimentary its organization, cannot be ascertained without the use of the humanistic coefficient. The gens, the tribe, the state, even the family and the horde have their being only in the experience and activity of their members, who have constructed them and now maintain them.

Sociology did not deny it. On the contrary, its intention was to study not only social groups as cultural products of the human beings included in a collectivity, but all the cultural systems existing within the sphere of experience and activity of these beings, made and

maintained by them. "Society" in the sense of Comte included the entire cultural life of the men belonging to it: language, art, religion, philosophy, science, technique, economic organization. Rooted in nature by their bodies, men were immersed in culture by their consciousness. And the culture of a society was common to its members; society, a mere collectivity of individuals from the natural point of view, was on its cultural side a superindividual entity, a *community* unified by sharing the same culture. All the cultural systems studied separately by special sciences were most closely intertwined in the cultural life of a "society," formed, in fact, a static and dynamic unity.

The successors of Comte preserved these essential presuppositions with but slight variation. Some ascribed more, others less importance to psychic as against material factors in the formation and existence of societies; some acceded to Comte's idea that the individual as a conscious being was entirely a part of society, and had no conscious life apart from what he shared with his society by sharing its culture; whereas others treated him as a psychological entity secondarily connected with "society" by communication and cooperation. Among the particular kinds of data constituting the culture of a "society," some were thought more, others less fundamental. While Comte had given predominance to intellectual factors, most sociologists emphasized, as St. Simon had already done, the importance of economic or technical phenomena; some, influenced partly by older doctrines, partly by the philosophy of Hegel, saw in the state system the supreme and determining phenomenon of "society."

It is not our intention to subject this sociological conception to systematic criticism at this point, for it has

been already judged by history. It has failed to produce a single valid and generally accepted scientific generalization concerning "societies": not one law which could be applied to the explanation and conditional prediction of the changes of "societies": no consistent classification of "societies," not even a general description of any particular class of "societies" which could be guaranteed to take into account all the essential characters common to that class and to no other. We do not mean, indeed, that sociologists working on this theory have not achieved any true, important, and exact scientific results. On the contrary, we believe that their works are greatly undervalued by the present generation of sociologists and that much useful knowledge is contained in them. But none of this knowledge concerns their main object-matter, "society": it bears upon what their authors thought minor matters, such as the structural characters and changes of specific groups or institutions, or particular socio-psychological processes.

The whole theory centered around the most striking fallacy. It identified two radically different and incommensurable concepts: "society" as a natural system of which the elements are individual animals of the species *Homo Sapiens*, and "society" as a combination of systems of which the elements are cultural values, like language, religion, technique, economic and political organizations, etc. That this obvious logical discrepancy did not attract the notice of those otherwise deep thinkers who created the great socio-philosophical "systems" of the past must be ascribed probably to the monistic current which prevailed in the scientific philosophy of the last century and carried these thinkers along.

But though the fundamental error was not discovered at once nor made explicit, even after it produced the usual result of scientific unproductivity, nevertheless the very progress of positive inductive research in neighboring fields has gradually corrected it by simply depriving that type of sociology of nearly all of what it conceived to be its proper object-matter, and dividing it—at least, as much of it as could be scientifically treated—among other disciplines.

On the one hand, indeed, all the positive and soluble problems bearing upon the natural aspect of human collectivities have been during the last sixty or seventy years appropriated by purely natural sciences, viz. human geography and physical anthropology, which are much better equipped than sociology for dealing with them efficiently. The separation of their fields from that of sociology is perhaps still imperfectly achieved because some geographers and somatic anthropologists show a tendency to "explain" cultural phenomena, while many sociologists are interested in geographic and racial questions. Yet, if we compare in this respect Gobineau with Ripley, or Demolins with Brunhes, we may confidently look forward to the time when the line of demarcation between anthropology and anthropo-geography on the one hand, and sociology and other sciences of culture on the other hand, will be drawn clearly and unmistakably.

Of course, there are innumerable vital problems concerning the relationship between natural and cultural systems, and these must be treated notwithstanding the division of the respective sciences. But it does not follow by any means that, as some defenders of the old conception of sociology as an intermediary between natural and cultural sciences wish to conclude,

these problems should, or even could, be the special privilege of one scientific discipline.[1] Far from it. These problems are so multiple and varied that it is absolutely impossible to reduce them to a common denominator. Every particular science handles different ones in the course of its research, and must deal with them on its own ground and with its own methods. The anthropo-geographic problems of mutual relationship between certain natural conditions and the formation of large cities are different and require different methods from those faced by a student of technology when, in investigating the development of a certain type of pottery, he tries to determine how much and in what way this development is dependent upon the geographical distribution of potter's clay. No general science of the connection between nature and culture could solve the problems which beset the linguist when investigating changes of pronunciation, the anthropologist studying the connection between racial mixtures, marriage and the caste system, and the religionist trying to determine what, if any, mutual dependence there is between certain forms of mysticism and certain physiological processes induced by the massing of numerous human bodies in closed buildings.

Thus, while the investigation of the natural aspect of human collectivities is gradually but completely passing from the hands of sociologists into the better qualified ones of geographers and anthropologists, a parallel process has lately been affecting the other, cultural side, of the nineteenth century "societies." The self-imposed task of sociology with regard to cultural

[1] This thesis is, I believe, the weakest part of the otherwise very valuable work of Sorokin, *Contemporary Sociological Theories*, New York, 1928.

communities has proved beyond its powers and, indeed, beyond the powers of any one science. For it meant nothing less than the comparative study of cultural communities, viewed in the total wealth and complexity of their civilizations, using the results of the historical and ethnographical studies which have been trying to describe in all relevant details the civilizations of particular communities, their techniques, their prevalent social and economic systems, their religion, language, literature, science, art and play. Sociology, from Comte on, has attempted to draw generalizations concerning all cultural communities, or at least all those belonging to a certain type.

Here again the actual development of scientific research has dealt a death-blow to such an undertaking, both by showing the impossibility of realizing it, and by substituting instead a different task, which is being fulfilled by a number of special sciences. In ethnography and the history of culture, as long as sweeping synthetic statements of a half-literary type concerning the civilization of particular peoples and nations prevailed over patient, thorough and critical descriptions of facts, comparative generalizations seemed not only possible, but easy. This was particularly true with regard to "primitive societies," whose civilizations appeared very simple in the light of the older ethnographical works (which, by the way, had still lower standards of thoroughness than those already developed at that time in history). But the more actual knowledge of concrete cultural communities increased in wealth and exactness, the more difficult it proved to organize into a synthetic rational picture everything known about the civilization of any particular community, however apparently simple, and the more doubtful appeared most of the

similarities on which sociologists (chiefly those of the evolutionary school) used to rely. The conclusion is inevitable that the total cultural life of any human community is much too rich and chaotic, contains too many heterogeneous cultural systems influencing one another in the most various and incalculable ways and is too ceaselessly and unexpectedly changing, to make valid scientific synthesis ever possible—which obviously precludes any comparative science of cultural communities.

There have been, indeed, some relatively limited and relatively stable combinations of various cultural systems recently discovered in the course of historical, prehistorical, and particularly ethnological research. These are the "cultural complexes" of the now predominant school of ethnology. Such a cultural complex does, indeed, contain definite technical, social, religious, aesthetic, economic systems interrelated in such a way as to make them usually appear together in the cultural life of human communities. But such a cultural complex is not coextensive with the civilization of any community, for every civilization we know contains several cultural complexes overlapping; and the ways they overlap, mix and influence one another are again most varied and incalculable. Moreover, there is no rational necessity, no static laws binding the various systems of a cultural complex together, connecting e.g. a particular religious system with a certain technical system. Nor is there any causal necessity, any universal dynamic law determining the origin and development of complexes and their expansion over certain cultural areas. The existence of any particular cultural complex and its acceptance, complete or partial, by certain cultural communities are simply historical facts which

happened once and will never happen again. This is the main reason why modern ethnology is historical as opposed to the earlier evolutionary ethnology, which assumed that the various cultural systems coexisting in a human community were necessarily dependent on one another, and that there were universal laws ruling everywhere the passage from one type of civilization to another. Historical ethnology has thus taken whatever wind there was out of the sails of sociology as a general theory of cultural communities—and it proved to be a weak breeze, merely allowing a careful sailing along the shore of historical facts, not a trade wind capable of driving the vessel of cultural science across the wide ocean of universal determinism.

There are still, however, attempts to revive this conception of sociology as a science of cultural communities. The chief argument in its favor is drawn from the obvious fact that the total cultural life of a community—a tribe, a nation, even a village, or a town —even though it does not constitute a higher kind of organic unity as the old sociologists believed, still is more than a mere sum of heterogeneous data, since its technique, economics, political organization, mores, religion, science, art, literature are closely intertwined and exercise a mutual influence. If there are special cultural sciences, each separately dealing with one of these domains of culture apart from its connection with all the other domains in the cultural life of human communities, should there not be a science investigating their inter-relationships? Its task may be difficult, but perhaps its failure heretofore is merely due to the application of wrong methods of research.

But while the premises of this argument are perfectly true, the conclusion is wrong. The mere fact of mutual

influence exercised by the various cultural systems upon one another is not enough to justify the existence of a distinct science studying this influence, for this task is already being performed by the several special sciences. All the technical, political, religious, scientific influences to which, say, economic systems are subjected in cultural communities must be investigated by the economists; the religionist has to take into account the modifications which a religion undergoes in consequence of economic, political and scientific processes going on in its cultural milieu, and so on. Something may, indeed, be left over after the various cultural systems composing the civilization of a human community have been taken into account. The people who share a certain set of interconnected systems (and among these systems there are usually also certain social groups—territorial, genetic or telic) may be more or less conscious of this fact, and more or less willing to influence one another for the benefit of their common civilization and to influence this civilization for their mutual benefit. This consciousness and willingness, in so far as they exist, constitute a social bond uniting these people over and above any formal social bonds which are due to the existence of regulated social relations and organized social groups. The reality of this bond is manifested in such familiar phenomena as public opinion, collective control of personalities and groups by their social milieu, development of new cultural ideals and attempts at their realization apart from organized group action. If the term "community" is limited to the humanistic reality embracing these phenomena, there is no doubt but that a "community" in this sense can be scientifically studied, and that sociology is the science to study it as one of the specifically social data. It is a

matter for discussion whether such a community is a social group or not, whether it is identical with the "public," whether it should be connected—as MacIver is doing—with territorial groups and nations.

Of one thing we may be sure, however, and that is that new efforts will be made continually to revive the old synthetic conception of sociology, for a powerful intellectual and moral interest is here in play. Every thinking man wishes to obtain some understanding of the totality of the civilization to which he belongs, compare it with other civilizations, interpret their history, discover if possible some guiding lines in the apparent chaos of the whole historical evolution of mankind. These interests are as undying and as justifiable in their way as the old metaphysical interest in interpreting the world of nature as some kind of ordered and rational whole. And there is an old and well-established discipline which satisfies them: it is the *philosophy of history*. We do not mean to deny its rights nor to belittle its importance. All we object to is having sociology, which aims to be a positive inductive science, exact and objective, so far misunderstand its possibilities and impossibilities as to undertake practically the same task. It was in part sociology's own fault that Paul Barth twenty years ago was able to republish the second edition of his voluminous work trying to demonstrate with much first-hand evidence that sociology, such as it had heretofore been, was the same as philosophy of history.[1] Of course, this view was one-sided and behind the times, for it failed to realize the significance of the new movements expressed chiefly in monographic research; but it may be considered symptomatic of the persistence of traditional ideals.

[1] *Philosophie der Geschichte als Soziologie*, Vol. I, Leipzig, 1914.

2. *Sociology as a General Theory of Cultural Data*

The tendency to become a science of culture in general, as against the special sciences like economics, linguistics and theory of religion, has expressed itself in still another sociological current, of a more recent origin. This current started in the two schools of Tarde and Durkheim which, with all their well-known opposition, have yet much in common, and it has since spread very widely, sometimes moving with the older current, sometimes resulting in important new variations. The common theoretic purpose of both Tarde and Durkheim,[1] their followers and associates, was not to reach a general theory of "societies" (although the concept of "society" remained as a general heuristic foundation of research, particularly with Durkheim), but rather a general theory of *cultural phenomena viewed as social phenomena.* The idea that every cultural phenomenon —technical, economic, religious, intellectual, linguistic —is essentially social, was founded in Tarde's view on the fact that its historical existence as something common to many people appeared due to interaction between human individuals, whose various forms Tarde summed up in his leading concept of "imitation," supplemented by that of "opposition." For Durkheim, however, the social character of the same cultural phenomena resulted from their being accepted by social groups as their values, and imposed upon the individual by the group to which he belongs. Under either assumption, sociology became the science which, by studying this common social foundation of all cultural phenomena, became the fundamental science of all

[1] The latter's first work on the *Division of Labor in Society* partly excepted.

culture,[1] of which other special sciences were meant to be variations or even mere subdivisions.

But, however interesting and even apparently convincing this conception of cultural phenomena as social phenomena might have been, the striking fact is that during the fifty years or thereabouts which have gone by since the first promulgation of the works of Tarde and of Durkheim, sociologists alone have become aware of the need of basing scientific research in the various fields of culture upon sociology, with the exception of the few, very few, religionists, economists and linguists who have become converted Durkheimians. This suggests either that all students of culture have been and still remain incomprehensibly blind to the logical relationship between their science and sociology; or else that, whatever sociology has to say to them, though by no means irrelevant—there are few specialists nowadays who entirely ignore sociological problems—is nevertheless not absolutely essential to the pursuit of their proper studies. The first solution of the puzzle is unthinkable, particularly since during these fifty years great progress has been made in most of the special sciences of culture, not under the leadership of sociology and often without much assistance from it. Thus, we must presume that the second solution is the true one.

And, indeed, if we do take into account a cultural system like a factory, a bank, a work of literature, a

[1] A similar rôle was ascribed somewhat later by Wilhelm Wundt to his "Völkerpsychologie." The latter, however, excluded those cultural systems which were primarily the products of individual activity and clearly manifested an objective, intrinsic order relatively independent of social influences—like science, philosophy, technical invention and literature.

system of religious dogmas and rites, a physical or mathematical theory, even though it is obvious that individual interaction, as emphasized by Tarde, was indispensable for its construction and remains indispensable for its maintenance; and even though it is usually accepted (as the school of Durkheim insists) by some group which sanctions its existence and in a sense vouches for its validity: nevertheless the system as such is *non-social* in its composition and structure in the sense that the individuals who work to construct it and maintain it are not its elements, nor is the group which supports it its structural basis. The factory, the bank, the religion, the work of literature, or the physical theory may remain exactly the same after all the individuals who participated in its maintenance leave or die and give place to others. The factory as a technical system does not necessarily change in its composition or structure by passing, say, from the control of a group of private capitalists to that of the state, nor a religion after a new nation has been converted to it, nor a physical theory in consequence of its being finally recognized in scientific circles, after having been for a while violently combated. There are cases, of course, when after such a change of participating individuals or supporting groups modifications of the system do follow, but these are directly due to the introduction of new technical instruments or processes, new religious dogmas or myths, new scientific concepts, for which the new men were perhaps responsible, but which might also have occurred while the system was still maintained by its former supporters.

This relative independence of the composition and structure of cultural systems from their social background makes a type of investigation possible which

ignores this background entirely, and such is the type that predominates in all special sciences of culture. A language may be and often is studied without any other knowledge of the people who use it than that they do speak it and understand it. A factory can be described exclusively in terms of materials, machines, methods, products, with no mention of the social life of the men who run it except that these men furnish the active forces needed to do so. A physical theory can be fully understood even if nothing is known about the personal life of the scientist who created it, his social relationship with his original opponents, or the organization of the scientific societies or congresses where it was finally approved.

When we do try to explain either the origins of a system or its later modifications, we must indeed take social factors into consideration even as other kinds of factors, natural or cultural. But it does not follow from the fact that social factors contributed to the composition and structure of Islam, of Shakespeare's *Hamlet*, of the Ford automobile factory, or of Einstein's theory that these systems are social, any more than the indubitable influence of geographic conditions in shaping the ritual of Islam makes it a geographic system, or the fact that money is needed for physical experiments makes them financial undertakings.

Now, while we doubt all possibility of a positive science of natural "societies" or cultural communities, we are far from denying that a general positive science of cultural phenomena—or, more exactly, of cultural systems—is possible. There are even, we believe, particularly in older philosophic literature, certain germs of it which can be developed into a science. If such

a general theory of culture is founded, then indeed all the special sciences will be dependent upon it, just as nowadays botany and zoology are branches of general biology. But it can be founded only by a slow process of induction in which the specific structural characters and changes of the systems constituting each particular domain of culture—technics, religion, art, economics—are investigated and compared with those of other domains. It is a tremendous task needing the cooperation of many specialists perfectly acquainted with their respective domains and at the same time able to rise above the limitations of their specialties.

3. Sociology as a Special Science

Having rejected the two main older conceptions according to which sociology should deal with all culture, either as a science of cultural "societies" or as a science of cultural phenomena in general—or rather, having simply accepted the unmistakable verdict the history of science has passed upon these conceptions—it still remains to show what the standards are that sociological research must apply in selecting its own data from the unlimited wealth and complexity of the empirical world. As a matter of fact, these standards need not be created. They are already implied in the successful, first-hand, positive investigation which has been carried on during the last forty or fifty years and whose results are embodied in thousands of monographs and systematic works. Most of this investigation bears the name of sociology in America, whereas in other countries, though the content is similar, it is often differently called; but the names are a minor

matter. The essential point is that these investigations bear only on a certain portion of the material the older sociological schools claim as their own; but this is a portion which even in those older schools was the object of particular interest and—what is more important—this material is not dealt with at all or only inefficiently by the established special sciences of culture, with one or two exceptions which will be pointed out later.

Attempts have already been made to formulate explicitly these implicit standards of selection of sociological data under the same assumption as ours, viz., that sociology is a special cultural science with an empirical field of its own; and if we try to improve on them by giving our own definition it is only because most of them seem still somewhat influenced by the older schools. This refers particularly to the conception of Simmel, according to whom all cultural phenomena have a social "form," though their "content" is not social but religious, economic, linguistic. This conception, though it has had great influence on the present German methodology (see Vierkandt) is misleading. For this "social form" of cultural phenomena does not manifest itself either in the composition or in the structure of cultural systems. At the same time, as Simmel and others have shown, it is something which can be empirically ascertained and studied apart from the systems of which it is supposed to be the "form." Therefore, it is obviously not a mere "form," but a specific class of empirical data accompanying various cultural systems in much the same way as theoretic reflection accompanies most of them in higher civilizations and religious beliefs and practices in earlier stages of culture. The actual object-matter of the sociological

research of Simmel was thus different from what he believed it to be.[1]

In comparing the specific data which have in fact been already appropriated by sociology to the complete or partial exclusion of other special sciences, we find that they easily fall into four main subdivisions.

4. *The Theory of Social Actions*

The first of these subdivisions is not only distinctly separated from various other kinds of cultural data—economic, religious, aesthetic, etc.—but investigations bearing upon it have already been in large measure systematized and constitute a particular discipline. We mean the so-called "social psychology" or, more exactly, that type of social psychology which has been officially initiated by McDougall, carried on by W. I. Thomas, Ross, Ellwood, Stoltenberg, Bogardus, Kimball Young; while others like Palante, and recently Vierkandt, treat it as an integral part of sociology. The data which furnish the material of this science can be briefly and provisionally described as actions bearing upon men as their objects and intending to provoke definite reactions on their part. They are *social actions,* clearly different from other actions which bear not upon men but upon material things, economic values, sacred objects and mystical powers, objects of aesthetic appreciation, linguistic symbols or scientific theories, and which intend to produce not human reactions but technical, economic, religious, artistic, literary, scientific results.

This difference, of course, does not prevent social actions from being often performed in a merely

[1] Cf. Theodore Abel, *Systematic Sociology in Germany.* New York, 1929.

auxiliary function, as when men fight for economic purposes or induce others to assist them in a technical work. Equally frequent is the opposite connection, when non-social actions are subsidiary to social ones: thus technical production may serve the purposes of a war of revenge; a man may gather wealth not for economic reasons but to obtain recognition from his neighbors; or a woman may go on the stage not for the sake of art but merely to gain fame. Similar relations exist, of course, between other kinds of actions.

We are not concerned at this moment with the heuristic concepts and methods used to study social actions; their popular reduction to psychological dispositions and processes ("instincts," "wishes," "responses," etc.) is a later problem. The fact is that they are being studied separately and more or less successfully, though improvements of method are always desirable and possible. But we must mention that under the name "social psychology," in addition to the one here mentioned, several other disciplines appear, whose data are not social actions but something entirely different. Thus, the studies of mob behavior made toward the end of the last century by Sighele, Le Bon and others gave rise to the idea that all collective actions, whatever their object and intention, are essentially different from individual actions; and "social psychology," in the sense of a psychology of collective or group behavior in general, became opposed to "individual psychology," as a psychology of individual behavior in general.

There is an obvious and fundamental, though not always clearly realized, conflict between this conception and the one advanced above, for instead of defining the actions by their objective aspect, their objects, re-

sults and methods, it defines them by their subjective source—the nature of the agent. Now, undoubtedly most actions performed collectively do differ from most actions performed individually, but not so much by their intrinsic character as by the fact that they are usually accompanied by social actions in the sense defined above. While a number of individuals perform a public prayer together, lynch a criminal, or produce nails in a factory, there are facts of social interaction going on between many of them individually, and between some of them—leaders and their opponents—and the whole mass.

But apart from these facts, which can be isolated and studied as social actions and reactions, there is obviously very little that the three collective actions here instanced have in common. Praying collectively in a church is a religious action and differs less from praying individually in a chapel than it does from producing nails collectively in a factory. The latter is a technical performance and is more akin to the performance of the blacksmith who makes nails alone in his forge than to lynching, which again is a social action distinguished only by secondary characters from the behavior of one or two individuals who take justice into their own hands and kill a man they judge worthy of death.

Instead, therefore, of treating these collective actions indiscriminately as data of the same science, whether it be called "social psychology," "collective psychology," "group psychology" or something else, thus putting them in a different category from the individual actions most like them, it is much more useful scientifically to study both collective and individual prayers as data of the theory of religion, both collective and individual production of nails as data of the theory of

technics, both collective and individual killing of criminals without due process of law as data of sociology. To the latter also may be left the investigation of those specifically social processes which may and usually do accompany the collective performance not only of social, but also of religious and technical actions.

The third conception of social psychology, radically diverging from both those we have discussed, is the one of Floyd Allport, Krueger, Reckless and others, in whose opinion the proper field of this science is, generally speaking, the bio-psychological human individual in so far as determined by the influence of the social milieu. We shall return to this matter presently when discussing the sociology of the personality. Obviously, however, a study of human individuals is a very different task from a study of social actions as defined above.

Finally, there is a fourth conception promulgated by Kantor,[1] who wishes "social psychology" to investigate individual "responses" to all kinds of cultural "stimuli" as distinct from responses to natural stimuli in which experimental psychology has been chiefly interested up to now. Social psychology in this sense becomes a naturalistic theory of cultural conduct, a part of the general psychological theory of the behavior of human individuals as bio-psychological entities.

This multiplicity of meanings which the term "social psychology" has acquired is an argument for the sociologist in favor of resigning it altogether and instead speaking simply of a "theory of social actions" as a branch of sociology. Though only lately circumscribed and systematized, this is the oldest branch of our science and indeed one of the oldest parts of human

[1] *An Outline of Social Psychology*, Chicago, 1929.

knowledge. Even putting aside the numerous generalizations scattered through the epic and dramatic literature of all ages and nations, we can trace its beginnings through philosophers and essayists like Nietsche, Schopenhauer, Hume, Schaftesbury, La Rochefoucauld, Descartes, Montaigne, St. Thomas, Machiavelli, Theophrastus, Aristotle, as far back as the primitive reflection embodied in popular proverbs.

5. *The Theory of Social Relations*

More recent are the origins of the second branch of sociology, whose nucleus is a comparative theory of moral rules, i.e., norms regulating social actions. While every thinking observer has a great variety of social actions given to him for comparison in his own social milieu, he must usually go to other societies to find rules different from those he finds recognized by his own society. And even then it is difficult to reach the purely objective point of view of theoretic reflection, since he is used to regard the morality in which he has been brought up as the only valid morality, a standard by which to judge other moralities rather than a datum to be compared with them. Sociological theories of morality had to be preceded by ethnographic descriptions, from which only about the seventeenth century (if we except the first inklings scattered in the *Essays* of Montaigne) comparative ethnological generalizations began to emerge. And in these, up to the last quarter of the nineteenth century, the moral rules prevailing among various peoples are indiscriminately mixed up with all other cultural data composing the civilizations of these peoples. Even as late as 1906, in Sumner's *Folkways* we still find under the same

denomination norms regulating the behavior of people with regard to other people, a number of purely technical, economic, religious rules, even principles of theoretic thought and aesthetic valuation.

Nevertheless, with the growing mass of ethnological material, specialization became imperative for monographic research. Norms regulating sexual life were the first to be studied separately, and have an enormous literature. Spencer's *Ceremonial Institutions* gives the first comparative outline of certain rules concerning the relations between the superior and the inferior. Of the latter, relations between masters and servants in their various historical forms have become the object-matter of the widest interest, owing to the growing social importance of the labor problem. The emphasis which social groups put upon particular rules of conduct was brought to light in studies concerning the treatment of offenders, of which the classical instance is Steinmetz' *Ethnologische Studien zur ersten Entwicklung der Strafe* (Leiden-Leipzig, 1894). Norms of solidarity between members of primary groups, particularly the various mutual obligations involved in kinship, have been investigated by many students of these groups, beginning with Morgan. Rules of peace and war between groups—since Grotius a matter of deep practical concern for students of politics—began to be investigated comparatively under the influence of those who, like Gumplowicz, applied to human groups the Darwinian conception of the struggle for existence.

The impulse toward separating this whole field and systematizing it has come, however, not from purely scientific interest, but from a philosophic opposition to traditional ethics. In order to show the vanity of all efforts to found rationally an absolute and universal

morality, the varieties and contradictions of moral rules actually recognized at various times and in various societies had to be systematically described and if possible explained as products of "natural," i.e., empirical and causally determined evolution: an "ethology" became opposed to "ethics." [1] The most comprehensive synthetic work in this line was and still is Westermarck's *Origin and Development of the Moral Ideas*, a very conscientious, though methodically imperfect contribution. Hobhouse's *Morals in Evolution* is more systematic, but too much imbued with the philosophy of the evolutionary school.

In selecting the data for this branch of sociology, we must keep in mind the distinctive character of moral rules as compared with all other rules: religious, economic, technical, intellectual, aesthetic. A moral rule, in the eyes of the subject—individual or collectivity—who recognizes it as valid and tries to act in accordance with it, appears as a duty which binds this subject with regard to some other individual or collectivity and which the latter expects him to fulfil. Now, there is always some other duty (similar or different) which this other individual or collectivity recognizes (or is at least expected to recognize) with regard to the subject, by which it feels bound and which the subject expects it to fulfil. In other words, social duties are always reciprocated by other social duties. If there are moral norms actually regulating the behavior of a woman to a man, a servant to a master, a subject to a king, a group member to his group, a church to a state, there are other norms with which, in his o

[1] For the exposition of this problem see Simmel, *die Wissenschaft*, Berlin, 1898, and Lévy-Bruhl, *La Morale et la science des mœurs*, Paris, Alcan.

sciousness and in that of the other party, the man has to comply in his conduct to the woman, the master to the servant, the king to the subject, the group to the member, the state to the church. The duties may be very different, their actual recognition and fulfilment unequal, but in principle there are no one-sided social duties. Every norm recognized by a social agent as his duty toward somebody else is a component of a social system in which this agent and the object of his duty are bound together as partners. We call such a system a *social relation*, and this whole branch of sociology dealing with moral data might be termed the *theory of social relations*.[1]

Other names have been suggested for that part of sociology which we are here discussing; *ethology* would be rather good, if the similarity of spelling to "ethnology" were not liable to produce misunderstandings. An apparent disadvantage of the use of our term is the existence of certain social norms which are recognized and followed by human agents with regard to everybody, not only to partners in social relations. But, as will be seen elsewhere, such norms are parts of personal or group ideals and presuppose another, higher level of organized social life than that which students of mores have usually investigated.

Although this field is indisputably sociology's own and no other science competes for it—ethics' interest being not theoretic, but normative—there has recently been much discussion as to the large domain which

[1] Many sociologists have given the term "social relation" a much wider extension; but the meaning in which it is used here is more in accordance with its established use in popular literature, from which we see no reason for departing. The matter will be discussed elsewhere.

borders on the field of moral facts: the domain of law. While in earlier societies there is no difference between law and morality, their distinctness is very apparent in every civilized society. At the same time it is obvious that in a large measure they do coincide: many legal norms formulate duties actually recognized by people in their dealings with other people in their own society, and thus constitute a ready material for sociological study of social relations. Now, there is a "science of law," much older than sociology and very firmly established, and some of its prominent representatives have shown a marked disinclination to having sociologists meddle with the data they consider their exclusive property.

There is, we believe, no need for any contentions or misunderstandings here, as far as the study of positive law is concerned. The student of morality is not interested in legal ordinances and statutes as rules imposed by the state upon its members. As long as he limits himself to social relations, legal rules are to him merely more or less adequate expressions of the duties actually recognized by the people of the given society in dealing with each other, eventually as factors influencing these duties. For instance, in studying the type of conjugal relations prevailing among the people who inhabit a certain country, he will assume provisionally that some—though not all—of the essential characters of these relations are expressed in the marriage laws. If he has other sources of information, he will try to test this assumption and find out whether there are any disagreements between the marriage laws and the data of "moral conscience" of married couples and their social milieu, since it is this "moral conscience," and not the law which is the real object of his interest. If

he investigates the changes which take place in conjugal relations within a certain community at a certain period, he must take into account marriage legislation as a possible factor of change, and try to follow up the effects it has had upon actual conjugal relations. Whereas the student of law, if he takes into account the connection between legal rules on the one hand, the "moral conscience" and the actual conduct of the people to whom these rules apply, on the other hand, is interested in it only from the point of view of the efficiency of these rules; law, not human conduct, is the primary object of his theoretic research.[1]

The divergence of standpoint is, of course, even more marked with those philosophic schools of jurisprudence whose primary interest, like that of ethics, is standardization and normative regulation. They are not concerned with the connection between law and human conduct as it really is; but, taking the efficiency of the law in controlling human conduct for granted, they try to determine what law is the best law for the promotion of human happiness, human progress, the welfare of the state, or whatever supreme standard of legislation they may accept. Of course, there is no possible conflict between this kind of legal doctrine and the sociological study of social relations, for there is no ground on which they can meet.

[1] This distinction is, of course, difficult to establish wherever data on human conduct are lacking and legal rules are the only source both for theory of law and for theory of mores. But this simply means that the student of mores must try to supplement such sources by looking for instances of actual behavior, whereas the task of the student of law is to connect legal rules with the structure of the state which gives them its sanction.

6. *The Theory of Social Persons*

The third branch of specifically sociological research is not yet circumscribed as distinctly as the preceding two, and its systematization is barely beginning. It deals, generally speaking, with the social aspect of the human personality as determined both by his social milieu and by his own activity. Every individual plays certain "social rôles," occupies certain positions and performs certain functions in his social environment, each involving definite rights and obligations, which in most cases are attached to similar positions and functions, and thus remain independent of him, although the way in which he actually realizes them and performs the social function corresponding to each position depends entirely upon himself.

Systematic theoretic investigation of social positions and functions was really started by Spencer in his "Professional Institutions"; certain parts of his "Political," "Ecclesiastical," "Industrial," and even "Domestic Institutions" have also a bearing upon this problem. Before him there were indeed many monographic studies made by historians and ethnographers of particular positions at certain periods and in certain societies, but few attempts at theoretic generalization. Most of the early generalizing reflection in this field is to be found in works of a practical, normative character outlining ideal requirements for various social positions and functions (kings, statesmen, judges, priests, warriors, philosophers, matrons, virgins), or else in criticism of the actual behavior of people functioning in certain positions.

The systematic attempt of Spencer has never been followed up; there is no general theory of social posi-

tions and functions. But since his time, monographic studies have been made in this field, such as the numerous comparative investigations concerning the shaman, or medicineman, and primitive chieftainship, the still more numerous studies on the social position of women, Frazer's famous theory of the king and the priest, Czarnowski's work on the hero, Sombart's on the bourgeois, Simmel's on the stranger and the poor. Recently much attention has been given to the position and functions of leaders, particularly political and economic leaders.

Since the individual is prepared for his social rôles in the process of education, which is specifically social in the strictest sense of the term, sociology has had to undertake the study of this process. However, the existence of an old and established discipline—the so-called educational theory or pedagogics, with its essentially practical trend—was a serious obstacle to the development of a purely theoretic and disinterested, comparative investigation of the facts of education as social data. Durkheim in his articles published posthumously under the title "Education et sociologie" outlined a program of such an investigation. Various historical studies of education and monographs on education among particular peoples have paved the way; but not until recently has the problem been taken in its fully sociological bearing. Krieck's *Menschenformung* and Lochner's *Descriptive Pädagogik* are the earliest comparative treatments of this problem; the present author's *Sociology of Education* (in Polish) is the first attempt at a complete sociological theory of educational facts. In connection, however, with the rapidly growing tendency to utilize the results of sociology for practical educational purposes, embodied in the so-

called "Educational Sociology," more and more monographic work of a sociological character is being done, particularly with reference to the school and the family as educational institutions.

The sociological problem of education is the intermediary link between the problem of the social position and function and that of the individual's own personality. And in studying the personality, separation of sociological from psychological and even biological questions seems very difficult. The earliest studies in this field bore primarily upon socially supernormal and subnormal personalities, i.e. those who gained positions of unusual social prominence or became social outcasts; and their social superiority or inferiority was conceived as being a result of their hereditary endowment. Galton's studies on *Hereditary Genius* and Lombroso's anthropological theory of the criminal are the best known instances to the point. The investigation of the relationship between the psycho-biological aspect of the individual and his social rôle, in which the latter is conceived as completely or partly a function of the former, has since become extended from uncommon to common human types. However varied may be the parts ascribed to heredity, early biological influences, education and social opportunity, respectively, this functional relationship remains the pivot of what has become an enormous and continually growing branch of research. Its growth has been, of course, tremendously stimulated by practical interests embodied in the great movements of eugenics and the physical and mental hygiene of the child.

In view of the fact that sociologists are interested in this problem as much as anybody else, I fear that they will not feel much sympathy for my conviction that the

dependence of the individual's social rôle on his psycho-biological characteristics is a matter which belongs fully and exclusively in the fields of psychology and human biology, and outside the reach of sociology. If sociology wants, it may try to solve the converse, but in a sense complementary problem: the dependence of the individual's psycho-biological characteristics on his social rôle. For, in my opinion, the sociologist must take the human individual not as he "really is" organically and psychologically, but as he is made by others and by himself to appear in their experience and his own in the course of his social relationships. From the sociological point of view, the primary matter about an individual is his social position and function, and this is not a manifestation of his nature, but a cultural system he constructs with the help of his milieu, seldom creating, usually copying it from ready models. The individual's organic and psychological features are from this point of view merely the material out of which his purely social personality, as characterized by the positions he fills and the way he fills them, has been formed in the course of education and self-education. Taking from Jung, and Park and Burgess, the conception that the essential point about a "person" is the idea he and others have of his social rôle, and remembering that the "idea" is not a mere mental picture, but a practical system of rights and obligations, we may thus call this entire branch of sociology the *theory of social persons.*

7. *The Theory of Social Groups*

The fourth and most developed branch of sociology is the *theory of social groups.* A group, of course, is

not a "society" in the old sense of the term: it is not an entity fully including a number of bio-psychological individuals and unifying them in a community of their total cultural life. It is simply one of the many cultural systems these individuals construct and maintain by their activities. There are, e.g., many thousands of various groups maintained by the inhabitants of a big city, from the municipal group in whose maintenance all of them participate, down to the small family groups kept up by only a few individuals each; and new groups are being constructed all the time. Every individual takes a more or less active part in maintaining a dozen or more various social groups of which he is a "member," i.e., in which he occupies certain positions involving definite rights and performs certain functions with definite obligations with regard to other members, to the group as a whole and to its functionaries. But his social life is not coextensive with his group life; on the contrary, the latter is merely a part of it, though a very important part. There are innumerable social actions he performs not as a member of any group, but simply as individual agent, like courting a woman, helping a passing stranger, breaking a municipal ordinance; there are a number of social relations to which he is a party and which have nothing to do with any of the groups he or the other party belongs to, like love, friendship, business partnership, subordination to an intellectual leader; there are social positions which do not necessarily involve the participation in any social groups, such as the professional positions of physician, painter, retail merchant, farmer. And, needless to say, the entire social life of an individual is only a part of his total cultural life: his technical, economic, intellectual, artistic, religious, hedonistic interests depend in some measure,

which varies widely with different men, on social contacts for their satisfaction, but are not specifically social. All the individuals agglomerated in a large city, besides participating in innumerable social groups, participate also in innumerable economic enterprises, construct and keep running innumerable technical mechanisms, help maintain a number of religious systems by accepting their dogmas and performing their rites, give the support of their understanding and appreciation to art, literature, science and philosophy. Often they form special groups or utilize existing groups for the purposes of more efficient economic, technical, religious, aesthetic, intellectual cooperation; but much, perhaps most of their common cultural activity, is going on outside of any group organization.

Social groups as specific cultural systems have been discovered by ethnologists and historians rather than by sociologists, who were absorbed by their attempts to build up a general theory of "society." Imperceptibly however, the sociologists' "society" began (in some schools at least) to shade off into the "group" or combination of groups; and theories like those of Gumplowicz and Ward may be considered typical of this period of transition. Nowadays, investigation of various types of groups is the common ground on which the ethnologist and the historian meet the sociologist and the political scientist.

In ethnology the need of separating the study of groups from the studies of other systems composing the total culture of lower communities arose out of the necessity of specialization. Already the ethnographer who describes a particular community must classify his material into separate divisions since, as we have seen, rational synthesis of a cultural community has proved

impossible. The ethnologist bent on comparative studies must specialize to some degree at least in some field of culture. Four main divisions can be distinguished in modern ethnology: one deals with material culture (often subdivided into technique and art), another with religion and magic, a third with language, a fourth with "social organization." The latter term includes what we call social relations and social persons (data on social positions and a few—very few—on education), but the materials investigated in this section of ethnology bear mostly on social groups. In history special studies of particular types of groups have been increasing with the development of monographic research, though, of course, one type of groups—states—have always been the predominant object of the historians' interest. Lately some sociologists have done, on their own initiative, independently of ethnologists and historians, first-hand research in some typical groups found in their social milieu.

We shall point out the most important kinds of groups on which there already exists good literature, without making any special distinction between the works of ethnologists and historians and those of professed sociologists; for when "social ethnology" and "social history" compare and generalize, they pass gradually into sociology.

Apart from the state to which we shall return later on, of all the groups those based on real or fictitious kinship—the family, the clan, the gens, the sib—have, as everybody knows, received the largest amount of attention. From McLennan and Morgan up to Rivers, Malinowski and Briffault, there is a long line of works dealing with various forms of family and kinship organization among lower communities, many of them

limited to certain ethnographic areas, but all implicitly or explicitly aiming at generalizations of a sociological character. Numerous comparisons of ethnographic and historical data like those of Morgan and Bachofen connect these studies with others made by historians. The Greek and Roman family, the German sib, the Indo-European or Aryan household have been the objects of special interest, and family life in the Middle Ages, the Renaissance and the modern period up to the French Revolution has not been neglected. Finally, there are monographs on the modern family in more recent times like Calhoun's *Social History of the American Family*, Thomas and Znaniecki's *The Polish Peasant* as well as innumerable less voluminous contributions made by sociologists working on first-hand materials. There are also comparative studies on the modern family in Western civilizations. And every synthetic work of the evolutionary school, every sociological textbook has a large section given to sociological theory of family and kinship organization in general. These groups form a practically indisputable part of the domain of sociology, though there are investigations lying on the borderland between this science and economics, like those done by Le Play and his school, who view the family as an economic, even more than a social system.

Among other groups in lower civilizations to which ethnologists and sociologists have given special attention are secret societies, age groups, and male groups: take Webster's study of *Primitive Secret Societies* and the work of H. Schurtz *Altersklassen und Männerbünde*. In so far as many secret societies have a religious character, there is some connection between these studies and others bearing more particularly upon religious

groups (either on certain particular historical varieties or on the whole class). See, for instance, Sighele *Psychologie des sectes.* To historians chiefly we owe a rich and first-rate literature concerning professional, caste and class organizations. Thus, mediaeval guilds have long been the subject of thorough research, and the type of professional group has been followed back to European antiquity and the Orient; something akin has even been discovered among lower civilizations (e.g., blacksmiths among the Massai in Africa). The caste systems of India are still something of a puzzle to sociologists as far at least as their origin is concerned. The aristocracies of various ages and countries —Polynesia, Greece and Rome, mediaeval knighthood, Western aristocracy of the last centuries before the Revolution—have attracted many a student. Still stronger has been the fascination exercised since the times of Voltaire by the clergy and its powerful organization in ancient Egypt and Babylon, in Buddhist countries, in mediaeval and modern Catholicism.

Particularly numerous and extensive have been the works concerning the past and present of the working classes. Interest was here stimulated, of course, by the modern labor movement. The great mass of literature bearing on this subject, however, has little scientific value, since it is dominated by practical, political or reformatory purposes. Objective theoretic investigation was at first made for the most part by economists who, even if they were thorough students in their own field, neglected or inadequately treated the purely social data involved in the processes they were studying. These social data, aside from the moral regulations of the duties of slave and owner, servant and master, employed and employer (which belong to the theory

of social relations) are primarily the social groups formed by people of the working classes and the struggles between these groups and others organized or dominated by the master or employer class. Only recently has this problem been clearly and definitely stated in a number of comparative sociological studies.

Very instructive have been the investigations of children's groups, such as those of Varendonck, Puffer and Thrasher. And even though we do not agree with the general presuppositions of the older "crowd psychology," still there are many valuable observations and generalizations contained in the studies of crowds and mobs by Sighele and Le Bon, and their followers. A marked progress in these studies is noticeable in the more recent works. The crowd or mob as a specific variety of social group, only half-formed and temporary, has an important place in the comparative theory of groups, since it throws much light on other, more durable and structurally closed social systems. For the same reason studies in other "primary groups," like the "congenial group," the "society circle," and of course the roaming horde (in so far as it is still real and accessible), must be highly valued.

During the Great War, Western sociology began to discover national groups. It had long been aware, of course, of the existence of nationalities or "races," as they were, and still are, popularly called; but a nationality is not a group, only a mass of people with common culture. Under the influence of political science and political history, sociology had commonly assumed that the social unity and organization of a nationality was only achieved in and through the state. The most striking instances to the contrary, such as the Polish national group, which remained socially unified though

belonging to three different states and actively struggling against all three, were but little known outside of Poland and Germany. The French and English languages do not even have any terms to indicate this type of group, for the term "nation" is associated with the idea of the state. Even at the present moment, though much work has already been done on the subject, the theory of national groups is very imperfect.

With the state, we approach the most disputed part of sociological data. The enormous prominence of politics in practical reflection and the great complexity of state systems have resulted in an early development of political science as a discipline of a normative character, partly philosophy, partly technology. The existence of this discipline has deeply influenced all theory of social data for many centuries. When sociology became conscious of itself as a distinct science, it was still imbued with the idea of the uniqueness and supremacy of the state among all social systems. "Civilized society" in the sense of Saint-Simon and Comte was coextensive with the state; the only important difference in their view as compared with earlier thinkers was that they no longer ascribed the predominant rôle in "society" to political activities, but took other cultural forces into account. In Germany until quite recently the state was distinguished from "society" as a superior organization embracing in a kind of higher unity all the social systems which could be found among the population of the state territory.[1]

[1] The problem of the political unification of Germany, which dominated German social activities in the past century, gave the practical impulse to this view; and Hegel's apotheosis of the state sanctioned it theoretically.

Gradually, however, the view began to dawn that states are merely a specific variety of social systems, more extensive and more complex than most (though there are religious groups still wider and with an organization not less involved), but not unique in a sense that would justify the traditional way of studying them apart, as if they bore no resemblance to any other social systems. They are obviously territorial groups like the village commune, the township, the neighborhood, the "local groups" of lower peoples. They are separate groups, not parts of other groups of the same kind, like most tribes, secret societies, religious groups of various denominations, national groups. They use coercion as means of social control in a larger measure than other groups—but few are the groups which entirely dispense with coercion. They influence and dominate many minor groups to which their members belong, but this is also a frequent feature of groups: the influence many a church, class organization or national group exercises upon minor groups is even more efficient. And, moreover, states in turn are often dominated by other groups—religious, professional, national. In short, since the study of social groups in general belongs to sociology, there is no valid scientific reason whatever why states as social groups should be excluded from this study or given a position incomparable with that of other groups.

There are, however, other reasons why sociology cannot completely absorb the so-called political science. Speaking of the latter, we must first distinguish two radically different disciplines current under this name and seldom clearly differentiated. One is a practical discipline, a technology of political activities; like all practical disciplines, it needs the help of several

theoretic sciences, such as economics, physical and human geography. It cannot be, therefore, identified with sociology any more than with any other theoretic science. The other is a comparative theory of states. Though such a theory logically is an integral part of sociology, it requires specialization in view of the enormous wealth of material it operates with (since this material includes all law, regarded not as a mere expression of moral norms, but as a constituent part of the state structure), and also because of its old tradition and vast literature. It is simply impossible to acquire proficiency in it without specializing extensively in this field. Similar specialization exists already in natural sciences; e.g., bacteriology has become a special division of biology. But a specialist in the theory of the state must be a sociologist, just as a bacteriologist is a biologist who has specialized in the study of bacteria.

There are as yet only a few explicit attempts to build a synthetic theory of social groups. Simmel in his *Sociology* deals with several varieties of groups and describes several characters and processes common to all; still, his work is a series of monographs rather than a general synthesis. In fact, however, nearly all recent general works bearing the title of "sociology" contain more or less systematic theories of groups, though many of them still partly confuse groups with "societies" in the sense of Comte and Spencer. This confusion can be avoided only by redefining the concept "society" from the point of view of these simpler systems with which monographic sociological research is dealing.[1]

[1] This is what, e.g., MacIver does when he calls a society "the web of social relationships," *Society, Its Structure and Changes*, p. 6. I think that a definition in terms of social groups is preferable.

8. General Definition of Social Systems [1]

This cursory survey of some modern developments in sociological research makes it perfectly obvious that there is a separate class of empirical data constituting the special field of sociology. Of course, these data can be either taken on their concrete historical background and studied in their individual peculiarities, or else isolated from this background and investigated with regard to their typical characters and repeatable changes: history and social ethnography are more interested in their first, analytic and nomothetic sociology in their second aspect; but, as we have already shown, the two aspects are complementary, and there is no sharp line of division between the task of the social historian or "sociographer" and that of the sociologist. The same relationship binds economic history with economics, history of art with theory of art, particular philologies with general linguistics.

It is, therefore, high time for the sociologists to drop the superannuated claims of making a "synthetic" or "fundamental" science of societies and culture, and to realize that whatever positive scientific results they can show to their credit have been achieved only by concentrating on those kinds of specific data we have characterized above as social actions, social relations, social persons, and social groups.

The logical reason for uniting these data within the domain of one science and separating this domain from

[1] Cf. the author's article, "The Object-matter of Sociology," in the *American Journal of Sociology*, January, 1927. The term "social systems" (with a somewhat different significance) was used by J. Boodin in an important article under that title in *Am. Journal of Sociol.*, Vol. XXIII, pp. 705-734.

those of other sciences is founded on the fact that all of them as cultural systems have an essential *similarity of composition*, while they differ in composition from all other cultural systems—technical, economic, religious, linguistic.

In order to prevent misunderstanding at this point, we repeat that the sociologist has nothing to do with human beings as natural entities, as they "really" are, individually or collectively, in their psycho-biological characters; he leaves their study to the psychologist, physiologist, anthropologist, human geographer. But in observing the *cultural world* he finds that in this world men play a double rôle. First, they are *conscious agents* or *active subjects*. An active subject as such is inaccessible to scientific observation; all we know about him objectively is that he is a center of experiences and activities. Consequently, there can be no positive science of "active subjects" or "conscious agents," but merely of their experiences and activities. Secondly, men are also empirical *objects of activity*, just as technical materials and instruments, economic values, works of art and literature, religious myths, words of language, etc. We have found that social actions differ from other actions in that they are dealing with men as objects; the same holds true of the other, more complex kinds of social systems. We should like to say in the language of the older psychology that human personalities, whatever they may be "in themselves," exist in the cultural world as "ideas" in the minds of other men, or as "representations" of other men; but it would be dangerous to use these terms, in view of their subjectivistic connotations. Therefore we prefer to say that they exist as *values* which active subjects experience and modify. We call them *social values* as distinguished from economic,

technical, religious, aesthetic, and other values. Man as a social value is only "an aspect" of himself as he appears to somebody else who is actively interested in him, or even to himself, when he reflects about his own personality and tries to control it practically. The sociologist, we might say, has only to do with such "aspects" of men. But these "aspects" are realities, since they really condition human behavior as much as, and sometimes more than, natural things and processes.

We cannot analyze here in detail the composition of social systems, for this is not a problem of methodology, but of positive classification and description; a few general indications will be sufficient at this moment to illustrate our main point. It has been seen already that in a social action an individual or a group is given to the agent as the "social object" whom the action is meant to influence. In a social relation there are two individuals or two groups, or an individual and a group, given to each other as "partners" of the relation, objects with regard to whom certain duties have to be fulfilled and from whom the fulfilment of other duties is expected. A "social person" is a center of relationships with a number of other persons and groups, in which relationships he appears as object of their activities and they appear as objects of his activities. A group is composed fundamentally of individual "members" each of whom is a social value for all the rest, the object of the collective assistance and control of the group as a whole, and all of whom cooperate in supporting the group as their common value. These objects have a common and distinctive feature which no other objects possess (unless they have been "anthropomorphized," assimilated to men in imagination); the agent is aware that they can

experience the same data he experiences and perform the same activities he performs.

Whatever other values enter into the composition of social systems are determined with reference to these fundamental or *primary social values*, are viewed as particular properties, forms of behavior, relationships of those who are the chief objects of active interest and in so far have the character of *secondary social values*. For instance, among the duties which enter into the composition of a social relation, there may be economic performances, as from master to servants, or religious performances, as from priest to layman. But within the relation both of them have a character of social rather than of economic or religious values, in so far as they are demanded, recognized, and appreciated as personal duties of the master or of the priest, implied in their social character as partners to the relation. Apparently equivalent economic or religious values coming to the servant or the layman from any other source would really be entirely dissimilar; being irrelevant to the social relation in question, they could not take the place of those which are due from the master or the priest. Other instances: the real estate owned by a municipal group, viewed apart from this group, is an economic value; the language of a national group, apart from its social structure, is a set of symbols, linguistic values. But in social activities bearing upon these groups, both acquire the character of social values as parts of these groups, as for instance when the city inhabitants view certain kinds of municipal property as an inalienable instrument necessary for the normal functioning of the municipal group which they are supporting, or when the members of a national group treat their language as a means for maintaining the national consciousness of

this group, essential for its separate existence as against other groups (even though for purposes of linguistic expression any other language would do as well or even better).

Assuming, then, that the systems we call social are composed of social values in the sense characterized above, we shall call *social tendencies* those active tendencies which are manifested in these systems. It might be mentioned at once that, social tendencies being only a specific variety of cultural tendencies, there are innumerable and multiform facts of interaction going on between them and other varieties of cultural tendencies. For instance, the formation of a social group may be counteracted by a divergence of economic interests or religious beliefs among its prospective members, or a group already formed may be disrupted by economic and religious conflicts appearing later; and, on the contrary, we often see social organizers appealing to common economic or religious tendencies in order to facilitate the formation of new groups, or statesmen trying to maintain the existing political system in spite of opposition by utilizing the tendencies of economic or religious conservatism. It is obvious, therefore, that a science dealing with social systems, though specializing in this field, cannot be isolated from other cultural sciences.

Another important point must still be raised with regard to this matter of scientific specialization. We claim that sociology is a special science because the composition of social systems is different from that of any other cultural systems. In this, we are following the example of other sciences of culture: the distinctions between economics, the theory of religion, the theory of language and the theory of art, are also based on

the fact that the composition of the respective kinds of systems differs—economic values are entirely dissimilar from religious values, the latter are distinct from linguistic and artistic values.

But are there no essential similarities of structure which might cut across the differences of composition? Thus, social actions, though dealing with different materials and instruments, reaching different results by different methods than technical, economic, religious actions, still—being actions—seem obviously to bear a strong structural similarity to them. A moral norm regulating the social behavior of an individual with regard to another individual appears formally, i.e., structurally, very much like a norm of utility with which his economic actions comply, a religious norm which regulates his acts of worship, an aesthetic or intellectual norm to which his mental activities are subordinated. This being the case, why should our entire scientific knowledge of the cultural world not rather be subdivided along entirely different lines; why should we not have, instead of sociology, economics, the theory of religion and the rest, a general theory of actions which would include social, economic and religious actions, a general theory of norms, and so on?

The reason is clear why similarities and differences of composition rather than those of structure constitute the ground of division between particular sciences of culture. The former are empirically given to any attentive observer at the very outset of his research, whereas the latter can be discovered only gradually in the course of research, and many of them remain still unknown. Thus, there is no need of any deep analysis to notice the resemblance between the simplest and the most complex religious phenomena on the one hand, the

simplest and the most complex economic phenomena on the other hand; and also to see the difference between these two classes. But the structural resemblance between a religious norm of sanctity and an economic norm of utility is much more difficult to discover, unless one sees it—as the schools of Durkheim and of Sumner do—in the purely external circumstance that both are usually sanctioned by social groups. While there are new data continually coming under scientific observation, the primary division of sciences of culture must be founded upon the character of the values composing the systems with which they deal. The investigation of structural similarities cutting across this primary division may become the task of a new group of sciences, or perhaps mere subdivisions of a new science —the general theory of culture, whose possibility we mentioned above.

References

The classical conception of sociology as a theory of "society" is developed in many works of which the most important probably are:

Comte, A., *Cours de philosophie positive*, Vols. IV to VI, (English translation, 1875).
Spencer, H., *Principles of Sociology*, Am. ed., New York, 1906.
Schäffle, A., *Bau und Leben des sozialen Körpers*, 2 ed. Tübingen, 1896.
Worms, René, *Organisme et société*, Paris, 1896.
Izoulet, *La cité moderne* (Alcan).
Durkheim, E., *De la division du travail social* (Alcan), Eng. Tr. by G. Simpson, New York, 1933.
Ward, Lester F., *Pure Sociology*, 2 ed., New York, 1911.

Giddings, F., *The Principles of Sociology*, 2 ed., New York, 1923.
Small, A., *General Sociology*, Chicago, 1905.
Sumner and Keller, *Science of Society*, Vols. I-III, New Haven, 1927.
Spann, O., *Gesellschaftslehre*, Leipzig, 1929.

Ross's *Principles of Sociology* (2 ed. New York, 1932) borders on this classical conception, though it is rather a study of the whole complex of specifically social phenomena occurring within a territorial collectivity.

MacIver's fundamental distinction between community, state, and society has done much to clear the traditional confusion. See:

MacIver, R., *Community*, Am. ed. New York, 1931.
——— *The Modern State*, 2 ed. New York, 1933.
——— *Society, Its Structure and Changes*, New York, 1931.

The following publications are the most significant for the conception of sociology as general science of cultural phenomena:

Tarde, G., *Les lois de l'imitation* (Alcan).
——— *L'opposition universelle* (Alcan).
——— *La logique sociale* (Alcan).
Durkheim, E., *Les règles de la méthode sociologique* (Alcan).
L'Année Sociologique, 15 Vols. published since 1896, Paris, Alcan.
Travaux de l'Année Sociologique (same publishers) including:
Lévy-Bruhl, L., *Les fonctions mentales dans les sociétés inférieures.*
——— *La mentalité primitive.*
Davy, G., *Le droit, l'idéalisme et l'expérience.*
——— *La foi jurée.*

Travaux de l'Année Sociologique (Cont.):

Durkheim, E., *Les formes élémentaires de la vie religieuse.*
Bouglé, C., *Essai sur le régime des castes.*
Hubert et Mauss, *Mélanges d'histoire des religions.*
Fauconnet, P., *La responsabilité.*
Czarnowski, S., *Le culte des héros.*
Halbwachs, M., *Les cadres sociaux de la mémoire.*
———, *Les classes ouvrières et les niveaux de vie.*

The conception of sociology as an independent science with a distinct field of data of its own has been approached and realized from various points of view by a number of leading workers. Their abstract definitions of sociology often diverge widely among themselves; some do not determine their positions distinctly, and hesitate between the idea of sociology as a general or synthetic science and sociology as a special science. This hesitation is mostly due to attempts to give sociology a biological foundation; and obviously biology can give no standards by which social phenomena could be distinguished from other kinds of cultural phenomena. Nevertheless, what really matters is the fact that, however divergent the general definitions or the theoretic standpoints, there is a main body of data actually investigated or delineated for investigation common to all of these works. With regard to this, some textbooks are significant:

Simmel, G., *Soziologie,* Leipzig, 1908.
——— *Grundfragen der Soziologie,* Sammlung Göschen.
Vierkandt, A., *Gesellschaftslehre,* 2 ed. Stuttgart, 1930.
v. Wiese, L., *System der allgemeinen Soziologie,* München, 1933.
Becker, H., *Systematic Sociology* (Translation and adaptation of v. Wiese's first ed.), New York, 1932.

Weber, Max, Cf. bibliography in Abel's *Systematic Sociology in Germany.*
Palante, G., *Précis de sociologie,* Paris, Alcan.
Richard, G., *La sociologie générale et les lois sociologiques* (Alcan).
Bouglé, C., *Qu'est-ce que la sociologie?* (Alcan).
Waxweiler, E., *Esquisse d'une sociologie,* Brussels, 1906.
Cooley, C., *Social Organization,* 2 ed. New York, 1914.
House, F., *The Range of Social Theory,* New York, 1929.
MacIver, R., *Society,* New York, 1931.
Eubank, E., *The Concepts of Sociology,* New York, 1932 (containing a well-chosen bibliography).
Bogardus, E., *Introduction to Sociology,* 4 ed. Los Angeles, 1928.
Park, R. and Burgess, E., *Introduction to the Science of Sociology,* Univ. of Chicago Press.
Groves, E., *An Introduction to Sociology,* New York, 1928.
Ellwood, C., *The Psychology of Human Society,* New York, 1925.
Dawson, C. and Gettys, W., *An Introduction to Sociology,* New York, 1929.
Duncan, H. G., *Background for Sociology,* New York, 1931.
This list might be considerably extended.

The conception of sociology as a special science here presented was first exposed by the author in his *Introduction to Sociology* (in Polish), Poznan, 1922, and later summarized in an article "The Object-Matter of Sociology," *Amer. Journal of Sociology,* 1927.

For social psychology as primarily a theory of social actions, see, for instance:

Sidis, Boris, *The Psychology of Suggestion,* New York, 1898.

McDougall, W., *An Introduction to Social Psychology*, 16 ed. Boston, 1923.
Duprat, G. L., *La psychologie sociale*, Paris, 1915.
Ross, E., *Social Psychology*, New York, 1923 (1st ed. 1908).
Stoltenberg, H., *Soziopsychologie*, Leipzig, 1914.
Ellwood, C., *Sociology in Its Psychological Aspects*, New York, 1912.
Thomas, W., *The Unadjusted Girl*, Boston, 1923.
Gault, R., *Social Psychology*, New York, 1923.
Bogardus, E., *Fundamentals of Social Psychology*, New York, 1926.
Young, K., *Source Book for Social Psychology*, New York, 1927.
——— *Social Psychology*, New York, 1930.
Williams, J. M., *Principles of Social Psychology*, New York, 1927.
Murphy, G., and L. B., *Experimental Social Psychology*, New York, 1931.
Lasher, B., *Race Attitudes in Children*, "The Inquiry," New York.
Holt, E., *The Freudian Wish*, 2 ed. New York, 1929.
Vaughan, W. F., *The Lure of Superiority*, New York, 1928.
Healy, Bronner and Bowers, *The Structure and Meaning of Psychoanalysis*, New York, 1930.
Martin, E., *The Behavior of Crowds*, New York, 1920.
Geiger, T., *Die Masse und ihre Aktion*, Stuttgart, 1926.

These instances have been selected so as to indicate their common ground which is active tendencies (or attitudes) bearing on people. Apart from this common ground, there are considerable divergences. Some of the works are concerned only with active tendencies of individuals, like

Stoltenberg's and McDougall's; others, like Martin and Geiger, only with those of collectivities; others still, like Young, include both. Studies of individual behavior border on and often pass into investigations of social persons: thus the works of Vaughan or Healy and psychoanalytic research in general. Whereas studies of collective behavior are closely connected with theories of social groups: thus, Le Bon's "crowd" is the nucleus of his group theory, while its main content is derived from the study of collective social actions.

For synthetic theories of mores and social relations, besides the works quoted in the text, see:

Maine, H. S., *Ancient Law*, London, 1891.
Dewey and Tufts, *Ethics*, New York, 1913.
Ross, E., *Social Control*, New York, 1920.
Cooley, C., *Human Nature and the Social Order*, New York, 1922.
Dewey, John, *Human Nature and Conduct*, New York, 1927.
Case, C., *Non-Violent Coercion*, New York, 1923.
Dorsey, George A., *Why We Behave Like Human Beings*, New York, 1925.
Hart, J., *Social Life and Institutions*, New York, 1924.
Judd, C., *The Psychology of Social Institutions*, New York, 1926.
Kulp, D., *Outline of the Sociology of Human Behavior*, New York, 1925.
Lumley, F., *Means of Social Control*, New York, 1925.
Malinowski, B., *Crime and Custom in Savage Society*, New York, 1926.
Briffault, R., *The Mothers*, New York, 1927.
Lowie, R., *Primitive Society*, New York, 1920.
Rivers, W., *Social Organization*, New York, 1924 (a study of early regulated relations rather than groups).

Sumner, Keller and Davie, *The Science of Society*, Vol. IV (materials), New Haven, 1928.
Frazer, F., *Totemism and Exogamy*, London, 1910.
Wallas, G., *Our Social Heritage*, New Haven, 1921.
Leopold, Lewis, *Prestige*, Am. ed. New York, 1913.
Carver and Hall, *Human Relations*, New York, 1923.
Hart, Hornell, *The Science of Social Relations*, New York, 1927.
Veblen, T., *The Theory of the Leisure Class*, New York, 1899.
Tönnies, F., *Die Sitte*, Frankfurt a/m, 1908.
Wundt, W., *Völkerpsychologie*, Vols. V and VI, *Die Sitte*.
Dupréel, *Le Rapport social*, Paris, 1912.

On the relation between the theory of mores and the theory of law, see:

Atkinson, *Primal Law*, New York, 1903.
Maine, H. S., *Ancient Law*, London, 1891.
Duguit, *Le droit social*, Paris, 1908 (tr. by F. & H. Laski as *Law in the Modern State*, New York, 1919).
Steinmetz, S. R., *Rechtsverhältnisse von eingeborene Völkern in Afrika und Ozeanien*, Berlin, 1903.
Kocourek, A., and Wigmore, J. H., editors, *Evolution of Law*, Boston, 1915.
Pound, R., *Law and Morals*, University of North Carolina Press, 1924.
Rolin, H., *Prolégomènes à la science du droit*, Brussels, 1911.
Cruet, J., *La vie du droit et l'impuissance des lois*, Paris, Flammarion, 1914.
Kohler, J., *Lehrbuch der Rechtsphilosophie*, Berlin, 1917 (tr. by Albrecht).
Ehrlich, E., *Grundlegung der soziologie des Rechts*, Munich-Leipzig, 1929.

Jerusalem, F., *Soziologie des Rechts*, Jena, 1925.
Kraft, J., "Rechtssoziologie," *Handwörterbuch der Soziologie.*
Dunkmann, K., Lehmann, G. and Sauermann, H., *Lehrbuch der Soziologie und Sozialphilosophie*, Berlin, 1931.

General theories of social persons are as yet nearly all founded on the presupposition that the study of the social person must begin with the determination of the psychobiological nature of the individual and then go on investigating the formation of the cultural personality under the influences of the social environment. Typical of this point of view are the following works:

Baldwin, J., *Social and Ethical Interpretations in Mental Development*, 3 ed. New York, 1902.
——— *The Individual and Society*, Boston, 1911.
Hall, G. S., *Adolescence*, New York, 1904.
Bechterev, W., *Die Persönlichkeit*, Wiesbaden, 1906.
Stern, W., *Die Menschliche Persönlichkeit*, Leipzig, 1923.
Kirkpatrick, E. A., *The Individual in the Making*, New York, 1911.
Thorndike, E., *Individuality*, Boston, 1911.
Adler, A., *The Neurotic Constitution*, New York, 1917.
Parmelee, M., *Personality and Conduct*, New York, 1918.
Groves, E., *Personal and Social Adjustment*, New York, 1923.
Allport, F., *Social Psychology*, New York, 1924.
Leighton, J. A., *The Individual and the Social Order*, New York, 1926.
Wells, G. R., *Individuality and Social Restraint*, New York, 1926.
Gordon, R. G., *Personality*, New York, 1926.
Krueger and Reckless, *Social Psychology*, New York, 1931.

Less influenced—some not at all—by naturalistic metaphysics are the following works:

Royce, J., *The World and the Individual*, New York, 1900.
Palante, G., *Combat pour l'individu*, Paris, Alcan.
Cooley, C., *Human Nature and the Social Order*, New York, 1902.
Hocking, W. E., *Human Nature and Its Remaking*, New Haven, 1918.
Jordan, E., *Forms of Individuality*, Indianapolis, 1927.
Burgess, E. W., editor, *Personality and the Social Group*, New Haven, 1918; Chicago, 1929.
Denison, G. H., *The Enlargement of Personality*, New York, 1930.
Eubank, E., *The Concepts of Sociology*, Chap. VII.
Litt, T., *Individuum und Gemeinschaft*, Leipzig, 1924.
Spranger, E., *Lebensformen*, Halle, 1924 (tr. *Types of Men*, 1928).

We can quote only a few instances of the monographic literature concerning specific social rôles which the individual performs in social life to illustrate the range of such studies; for we shall have to utilize this literature more in detail elsewhere:

Frazer, J., *The Golden Bough*, London, New York, 1907-15 (especially for the king and the priest).
Briffault, R., *The Mothers*, New York, 1927 (for religious and family rôles of women).
Mason, O. T., *Woman's Rôle in Primitive Culture*, New York, 1927 (particularly for technical functions).
Thomas, W. I., *Sex and Society*, Boston, 1909.
Carlyle, T., *On Heroes, Hero-Worship and the Heroic in History*, Boston, 1840.
Czarnowski, S., *Le culte des héros*, Paris, Alcan.

Walter, F., *Die Propheten in ihrem sozialen Beruf*, Freiburg i. B., 1900.
Smith, J. M. P., *The Prophet and His Problems*, New York, 1914.
Iles, G., *Inventors at Work*, New York, 1906.
Mumford, E., *The Origins of Leadership*, New York, 1909.
Ostwald, W. (editor), *Grösse Männer*.
Taussig, F. W., *Inventors and Money Makers*, New York, 1915.
Thurnwald, R., "Führerschaft und Siebung," *Zeitschrift f. Völkerpsych. u. Soziologie*, 1926.
Lange-Eichenbaum, W., *Genie, Irrsinn u. Ruhm*, Munich, 1928.
Hamon, *Psychologie du militaire professionnel*, Brussels, 1894.
Sombart, W., *Der Bourgeois*, Munich, 1913.
——— *The Jews and Modern Capitalism*, New York, 1913.
Thomas and Znaniecki, *The Polish Peasant*, New York, 1927.
Sombart, W., *Das Proletariat*, Frankfurt, 1900.
Broda and Deutsch, *Das moderne Proletariat*, Berlin, 1910.
Hammond, J. L. and Hammond, B., *The Town Laborer*, 1760-1837, London, 1917.
Hauser, H., *Les ouvriers des temps passés*, Paris, Alcan.

Here belong also, in part, the innumerable works on the socially subnormal and outcast: the criminal, the vagabond, the prostitute, the dependent. We say in part, for the majority of these works are more concerned with the logical and psychological dispositions of the socially subnormal than with their social rôles.

Objective studies of education, as a process by wh

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individuals are being prepared for their social rôles, were really initiated by ethnologists and historians of culture. The classical compilation of H. H. Ploss, *Das Kind in Brauch und Sitte der Völker* (first ed. Leipzig, 1884) opened the way. Since then, a number of studies of the child and its education in lower societies have been published, such as: Spencer, T., *Education of the Pueblo Child*, New York, 1899; Eastman, C. A., *Indian Boyhood*, New York, 1902; Kidd, D., *Savage Childhood*, London, 1906; Miller, N., *The Child in Primitive Society*, New York, 1928. This led, naturally, to systematic objective research in modern childhood and education, as exemplified by such works as W. I. Thomas and D. S. Thomas, *The Child in America*, New York, 1928, and v. Wiese (ed.), *Soziologie der Volksbildung*, Berlin, 1921 (on adult education). P. Barth in his book *Geschichte der Erziehung*, Leipzig, 1911, introduced the sociological point of view into historical studies of education.

We cannot quote here educational books with normative purposes which as a basis for practical ideals contain objective theories of the educational process as a formation of the social person. The need for such a basis has become very general and there are valuable contributions to the sociology of the person contained in the works of such men as Dewey, Kilpatrick, W. Bagley, W. F. Russell, Counts, Snedden, Kerschensteiner, Spranger, Compayré, Piaget, Nawroczynski, Szuman, and many others. These contributions, which must be taken into account in any study of the social person, we leave for future reference, as well as the numerous monographs which have recently multiplied in connection with the progress of research in higher institutions for the training of educators. We shall mention only a few samples of works which are explicitly meant to be primarily theoretic rather than prac-

tical and have a synthetic rather than a monographic character:

Krieck, E., *Menschenformung*, Leipzig, 1927.
Lochner, *Deskriptive Pädagogik*, Reichenberg, 1927.
Blanchard, P., *The Child and Society*, New York, 1928.
Waller, *The Sociology of Teaching*, New York, 1932.

General theories on social groups are mostly included in systems of sociology. See, for example:

Gumplowicz, L., *System of Sociology* (in Polish) or *The Outlines of Sociology*, Philadelphia, 1899.
Durkheim, E., *De la division du travail social*, Paris, Alcan.
Giddings, F. H., *The Principles of Sociology*, New York, 1896 (new ed. 1923).
——— *Descriptive and Historical Sociology*, New York, 1906.
Simmel, G., *Soziologie*, Leipzig, 1908 (new ed. 1922).
Ellwood, C. A., *The Psychology of Human Society*, New York, 1925.
Vierkandt, A., *Gesellschaftslehre*, 2 ed. Stuttgart, 1930.
v. Wiese, L., *Allgemeine Soziologie*, Munich, 1933.
Bogardus, E. S., *Introduction to Sociology*, Los Angeles, 1928.
Good, A., *Sociology and Education*, New York, 1926.
MacIver, R., *Society, Its Structure and Changes*, New York, 1931.

Considering, however, the importance of the group as the most comprehensive social system and the fact that most sociological works deal chiefly with problems concerning group life, it is curious how little place discussions of group life in general take in textbooks of sociology, for instance. Thus, Park and Burgess devote one chapter to "Society and the Group"; similarly, Reuter and Hart in their

recent *Introduction to Sociology* (New York, 1933). Works dealing explicitly with the general problem of groups are also rather scarce. We may quote:

Maunier, R., *Essai sur les groupements sociaux*, Paris, 1929.

McDougall, W., *The Group Mind*, 2 ed. New York, 1920.

Solms, M., *Bau und Gliederung der Menschengruppen*, Karlsruhe, 1926.

Brown, B. W., *Social Groups*, Chicago, 1926.

Coyle, G., *Social Process in Organized Groups*, New York, 1931.

And a few articles, such as:

Bodenhafer, W. B., "Comparative Rôle of the Group Concept in Ward's Dynamic Sociology and Contemporary American Sociology," *Amer. Jour. of Sociology*, Vol. XXVI.

Allport, F. G., " 'Group' and 'Institution,' " *Publ. Amer. Sociological Soc.*, 1928 (criticism of the group concept).

Sapir, E., "Group," *Encyclopedia of the Social Sciences.*

v. Wiese, L., "Allgemeine Theorie der Gruppe," *Jahrbuch für Soziologie*, III (1927).

Whereas the literature on specific varieties of groups is enormous. See, e.g. On "primary groups" in general:

Cooley, C. H., *Social organization*, 2 ed. New York, 1914, Chap. III ff. (Cooley introduced this concept into sociology).

Clow, F. R., *Principles of Sociology with Educational Applications*, New York, 1920, Chap. V.

Thomas and Znaniecki, *The Polish Peasant*, Part I, "Primary-Group Organization."

Kolb, J. B., *Rural Primary Groups*, Madison, 1921.

Bernard, L. L., *An Introduction to Social Psychology*, New York, 1926, Chap. XXVI.
Faris, E., "The Primary Group: Essence and Accident," *Am. Jour. of Sociol.*, 1932.

On the family and genetic groups:

Morgan L., *Ancient Society*, 1st ed. 1877.
Le Play, P. G. F., *L'organisation de la famille*, Paris, 1871.
Kovalevsky, M., *Tableau des origines et de l'évolution de la famille et de la propriété*, Stockholm, 1890.
Grosse, Ernst, *Die Formen der Familie und die Formen der Wirtschaft*, Freiburg i. B., 1896.
Thwing, C. F. and Carrie F. B., *The Family*, Boston, 1913.
Bosanquet, H., *The Family*, London, 1915.
Dealey, J. Q., *The Family in Its Sociological Aspects*, Boston, 1912.
Goodsell, W., *A History of the Family*, New York, 1926.
Mowrer, E. R., *The Family*, Chicago, 1932.
Briffault, R., *The Mothers*, New York, 1927.
Reuter and Runner, *The Family*, New York, 1931.
Malinowski, B., *The Family Among the Australian Aborigines*, London, 1913.
——— *The Father in Primitive Psychology*, New York, 1927.
Hearn, W. E., *The Aryan Household*, London, 1879.
Rivers, W. H. R., *Kinship and Social Organization*, London, 1914.
Smith, W. Robertson, *Kinship and Marriage in Early Arabia*, Cambridge, 1885.
Lowie, R. H., *Primitive Society*, New York, 1920.
Thomas, N. W., *Kinship Organizations and Group Marriages in Australia*, Cambridge, 1906.

Westermark, E., *The History of Human Marriage*, New York, 1921 (belongs rather to the theory of social relations than to group studies).
Calhoun, A. W., *Social History of the American Family*, Cleveland, 1917-19.
Ogburn, W. F. and Tibbitts, C., "The Family and Its Functions" in *Recent Social Trends in the United States*, Vol. I, New York, 1933.

For territorial groups (other than the state) see:

Maine, H., *Village Communities in the East and West*, 7 ed. London, 1913.
Seebohm, F., *The English Village Community*, London, 1883.
Baden-Powell, H., *The Indian Village Community*, London, 1896.
Galpin, C. J., *The Social Anatomy of an Agricultural Community*, Madison, 1915.
Sims, N. L., *The Rural Community, Ancient and Modern*, New York, 1920.
Brunner, E. de S., *Village Communities*, New York, 1927.
Steiner, J. S., *The American Community in Action*, New York, 1928.
Sanderson, D., *The Rural Community*, Boston, 1932.
v. Wiese, L., *Das Dorf als soziales Gebilde*, Munich, 1927.
Preuss, H., *Die Entwicklung des deutschen Städtewesens*, Leipzig, 1906.
Pirenne, H., *Les villes du Moyen Âge*, Brussels, 1927.
Park and Burgess, *The City*, Chicago, 1925 (contains a bibliography).
Anderson and Lindeman, *Urban Sociology*, New York, 1928.
Carpenter, N., *The Sociology of City Life*, New York, 1931.

Sorokin and Zimmerman, *Principles of Rural-Urban Sociology*, New York, 1929.

Most of the above works, however, tend to include in the study of the rural or urban territorial group much too great a complexity of social and generally cultural phenomena. I have tried, on the contrary, to give a more limited, strictly sociological rather than anthropo-geographic conception of the city as a group in two monographs:

Znaniecki, F., "Group Crises" in K. Young (editor), *Social Attitudes*, New York, 1931.
——— *The City in the Consciousness of Its Inhabitants* (in Polish), Poznan, 1931.

Here are a few instances of monographic studies of other specific groups:

Howitt, A. W., *The Organization of Australian Tribes*, London, 1888.
Wheeler, C. G., *The Tribe and Inter-Tribal Relations in Australia*, London, 1910.
Boas, F., *The Social Organization and the Secret Societies of the Kwakiutl Indians*, Reports of the U. S. Natl. Museum, Washington, 1897.
Schurtz, H., *Altersklassen und Männerbünde*, Berlin, 1902.
Webster, H., *Primitive Secret Societies*, 2 ed. New York, 1932.
Heckethorn, C. W., *The Secret Societies of All Ages and Countries*, London, 1897.
Schuster, G., *Die geheimen Gesellschaften*, Leipzig, 1900.
Brentano, L., *"Gewerkvereine," Handwörterbuch der Staatswissenschaften.*
Rademacher, A., *Die Kirche als Gemeinschaft und Gesellschaft*, Augsburg, 1931.

Sighèle, S., *Psychologie des sectes*, Paris, 1898.
Durkheim, E., *Les formes élémentaires de la vie religieuse*, Alcan.
Varendonck, *Recherches sur les sociétés d'enfants*, Brussels, 1914.
Puffer, J. A., *The Boy and His Gang*, Boston, 1912.
Thrasher, F., *The Gang*, Chicago, 1927.
Furfey, P. H., *The Gang Age*, New York, 1926.
Webb, S. and B., *Industrial Democracy*, New York, 1914.
Marot, Helen, *American Labor Unions*, New York, 1914.
Michels, R., *Political Parties*, New York, 1915.

In the following works on crowds, masses and mass movements, social classes and races, the distinction between groups in the strict sense of the term, as closed social systems, and collectivities of individuals with similar tendencies and common interests, is seldom clearly drawn. Nevertheless, they are contributions to a theory of groups, inasmuch as groups often do emerge from such collectivities or represent them in action. See:

Le Bon, G., *The Crowd* (Eng. tr.), London, 1922.
Geiger, T., *Die Masse und ihre Aktion*, Stuttgart, 1926.
Trotter, W., *Instincts of the Herd in Peace and War*, London, 1916.
Conway, M., *The Crowd in Peace and War*, New York, 1915.
Freud, S., *Massenpsychologie und Ichanalyse*, Vienna, 1923.
Bauer, A., *Les classes sociales*, Paris, 1902.
Veblen, T., *The Theory of the Leisure Class*, New York, 1924.
Fahlbeck, P. S., *Die Klassen und die Gesellschaft*, Jena, 1922.
Bouglé, C., *Essai sur le régime des castes*, Paris, Alcan.

Miller, H. A., *Races, Nations and Classes*, New York, 1924.

The sociological approach to the state and political reality in general is represented by a number of prominent works. Older literature is summarized in H. E. Barnes, *Sociology and Political Theory*, New York, 1924. The most important contributions since then are:

MacIver, R. M., *The Modern State*, London, 1926.
Catlin, G. E. C., *Science and Method of Politics*, New York, 1927 (discussed by W. Y. Elliott in *Methods in Social Science*, pp. 82-94).
——— *A Study of the Principles of Politics*, New York, 1930.
Staudinger, F., *Kulturgrundlagen der Politik*, Jena, 1914.
Oppenheimer, F., *System der Soziologie*, Vol. III, *Der Staat*, Jena, 1929.
Weber, Alfred, *Ideen zur Staats-und Kultursoziologie*, Karlsruhe, 1927.

We should like to cite many more works in this bibliography as the most obvious proof that the field of specific data that are actually being investigated by sociology is identical with our definition. For the only kind of data actually studied by sociologists which are not social systems as here characterized are either "social processes," reducible to changes of these systems (Cf. our "Analysis of Social Processes," Publications of the Am. Sociol. Soc. 1933), or else are not distinct data, but complexes of data belonging already to a number of other sciences. But since this is not meant to be a bibliographical work, we hope that the instances quoted are sufficient to support our contention that sociology is a special science with a limited domain of reality of its own.

CHAPTER IV

The Sources of Sociological Material

1. *Inadequate Utilization of Sociological Material*

Sociology, as we have already pointed out, is only beginning to learn how to make proper use of its material, which is being agglomerated at a tremendous rate. While many older sociologists on a slender foundation of fact built imposing speculative constructions, which crumbled down before they were finished, we are heaping up mountains of raw stuff and barely manage to raise on top of them small and unsightly shreds of timid theory. We dignify this procedure by ascribing it to scientific circumspection, whereas in most cases it is nothing but plain incapacity to do any better.

The actual value of any material depends on the purpose for which it is used and the way it is handled. The range of possible purposes for which a given material can be used is always limited; but it may be narrower or wider, depending on the relative efficiency of the instruments and methods of handling it. The specific purposes of scientific research as distinct from other kinds of activity are to solve theoretic problems by formulating and testing hypotheses; these purposes are set in stating the problems for solution. An inductive science like sociology, which has certain kinds of material given to it, should only state such problems as this material will allow it to solve; but the range of

problems soluble with the given material depends on the way this material is handled, i.e., the way in which hypotheses concerning it are formulated and tested.

Our present incapacity to utilize given sociological material for scientific purposes means that the range of sociological problems which are considered soluble with any given material is too narrow. We lack the theoretic vision of the physicist or the biologist. And the reason is not merely that we have become too critical in reacting against too much vision on the part of the great "fathers of sociology." The main factor which has produced this mental narrowness is simply that kind of unreflective philosophy which makes the majority of present sociologists identify the objective with the sensually given, and ignore the obvious existence of two distinct objective worlds: nature and culture.[1] In consequence of this, most sociologists are feeling apologetic about the type of material they use. With rare exceptions, it does not even occur to them that, owing to those very characters which distinguish this material from that of natural science, it may contain as good, though different, potentialities of valid theory as the latter; and they hardly ever try to learn how to realize these potentialities more efficiently. On the contrary, the most conscientious ones usually tend to reduce this material as nearly as possible to the naturalistic type and to use only those fragments or aspects of it which approach

[1] The best corrective for this stunted view would be, in my opinion, becoming acquainted with the opposite, idealistic metaphysics, particularly Kant, Fichte, Hegel and Schopenhauer or their modern disciples. While equally dangerous, if used as a foundation for scientific research, it has at least the advantage of being the logical result of intelligent, though one-sided reflection about mind and reality.

this type. And if a sociologist is found bold enough to treat unashamedly sociological material as distinct from (let us say) biological material, he is immediately proclaimed a "social philosopher" and condemned as such.[1]

Let us make a survey of sociological materials, keeping firmly in mind the general premises about cultural systems outlined in Chapter II, and having all the time in view the question as to what lines of approach this given kind of material offers for sociological analysis.

The sources from which sociology actually draws its materials can be classified into:

a) Personal experience of the sociologist, original and vicarious;
b) Observation by the sociologist, direct and indirect;
c) Personal experience of other people;
d) Observation by other people.

To these main sources must be added, as a subsidiary source

e) Generalizations made by other people with or without scientific purposes.

Though it would be difficult to draw sharp lines of division between materials coming from these various sources, in every particular case it is possible to determine the prevalent character of the given material and to treat it accordingly.

[1] I used to be indignant at being thus qualified until, on comparing notes, I found that the same reproaches had been addressed to men like Tönnies, Simmel, Vierkandt, Weber, Tarde, Durkheim, Small, Giddings, Ross, Park, MacIver. Now I am only proud to be in such company.

2. *Personal Experience of the Sociologist: Original*

The scientist's personal experience is the primary and most reliable source of information in sociology, just as in every other science, though it is naturally limited. Its reliability as well as its limitations seem so obvious that it is difficult to understand how there ever could have appeared schools of scientific thought which either reject it altogether or else extol it as an entirely self-sufficient source of sociological data.

The only way actually to experience a social system at first-hand is to be active in its construction, for only thus are we directly aware of the tendencies involved in its structure and the actual significance of the values included in its composition. Just as we cannot fully experience a sentence without speaking it, a game of golf without playing it, a geometrical theorem without demonstrating it ourselves, even so it is impossible to experience fully the social actions of avoiding, intimidating, helping, sympathizing or demanding sympathy, acquiring mastery or gaining recognition otherwise than by performing such actions; or to experience a friendship without being friends with somebody, a conjugal relation without being married, a position of teacher, student, stranger, workman without occupying it, the composition and structure of a gang, a "secret society," a church, a national group without participating in it.

On the other hand, of course, the individual sociologist cannot experience himself, at first-hand, all the social systems he has to study, for not only would his field of data be then very limited, but most of the data would be given to him only partially and imperfectly, from a necessarily one-sided point of view, since the

majority of social systems require for their construction the collaboration of several individuals. Thus, even among social actions there are some that no individual can adequately reproduce: take the election of a public official. And it is obvious that the individual's experience of a friendship or a conjugal relation must be supplemented by the experience of the other partner. Moreover, the social life of a group will be differently experienced by members who occupy different social positions within this group or take an unequal personal share in its activities.

But after recognizing these limitations, the sociologist must claim for his own social experiences, as far as they go, the same objective validity as students of nature claim for their methodical observations. Of course, both the sociologist and the student of nature take risks of error, and perhaps with the sociologist, these risks are greater; but they can be avoided and, if they are, the evidence in both cases is equally conclusive.

The mistrust often shown toward the use of the sociologist's own experiences for scientific theory can be directly traced to a mistaken analogy between social or, more generally, cultural experience and psychological "introspection." The mistake originated in that old dualistic philosophy which assumed two kinds of experience: "external" or sensual, and "internal" or psychological. Modern materialism, having for its own purposes identified valid experience with sensual experience of natural data, thinks of all the experiences it cannot use in terms of introspective psychology. But the experience of cultural systems is no more "internal" than the experience of natural systems—nor is either of them "external." The difference does not lie in the character of the "experience" in the sense of the process

of experiencing, but in the character of the data which are experienced and which in one case include systems of things, in the other systems of values. Consequently, the reproach of "subjectivity" which has been formulated against psychological "introspection" as a source of knowledge is not applicable to cultural experience. The defects of the method practiced by introspective psychology justify indeed this reproach; but the student of culture can avoid them.

In order to turn any of our experiences into knowledge we must "reflect" about them, that is, analyze that which is experienced and reconstruct it theoretically. We are doing this, for instance, when in experiencing a tree or a laboratory process, we try to describe the data of our experience for scientific purposes, turning our reflective attention to the size, shape, various aspects and elements of the tree or the physical process as we see it and the way these aspects and elements combine. In the same way, in order to utilize scientifically our experience of a hostile act we have performed or a relation of friendship in which we participate we must reflect about it, turn our attention to the particular values with which we were or are dealing and the way we did tend or are tending to combine and to modify these values.

In both cases reflecting about experience is different from experiencing: it is a specific kind of activity—the primary form of theoretic activity. This activity in one case bears upon the experience of a tree or a laboratory process, which is a natural system; in the other case, upon the experience of hostile behavior or the relation of friendship, which is a cultural system. In both cases, theoretic activity utilizes the data of experience as material for theoretic construction, modifies and reorgan-

izes them for its own purposes; and in so far there is no essential difference between these cases.

Now, introspective psychology has tried to develop the technique of "self-analysis." And the trouble with the "self-analyst" is that he is not satisfied with reflecting theoretically about the experience he actually had or is having while constructing a cultural system. He searches for data which were not or are not actually present in his experience, for "hidden motives," "unconscious complexes," etc. In other words, instead of using for his reflection the facts as he finds them, he makes up new facts in the course of his reflection. Of course, he imagines he is only discovering facts that were there but escaped his notice at first. And this is really the crucial point. For the existence of a cultural system being essentially kinetic, reflection about it must rely on memory in a larger measure than reflection about most natural systems. Still, it is only a difference of degree. Natural systems change, sometimes very rapidly; in describing natural processes, actual experience and memory must cooperate in various measures. The particular sciences of nature have had to work very long trying to improve their technique so as to diminish the errors that originate in the distortion of facts by memory. And the first thing they did was to eliminate as far as possible the main source of this distortion—*practical interest* of the student in the data he tries to remember. The physicist or biologist has learned to suppress, while engaged in a theoretic reflection about past as well as present experiences, all tendencies and valuations concerning their object-matter, to keep a detached, purely theoretical attitude toward it.

There is no reason why a sociologist (or any other humanist) should not preserve the same theoretical at-

titude while reflecting about his own experiences, even though the latter are essentially practical; and he can do it more easily when these experiences are past, since, being no longer active socially, he can concentrate entirely on theoretic study. But he must be well aware of the difference betwen that which he is sure of having consciously experienced when he was active, and anything that may occur to him when he tries to understand *now* his past activity. Only what he explicitly remembers as part of his active experience can be treated as fact; and he must often resign himself to fragmentary and incomplete facts. For whatever he may think of later to supplement his explicit memory under the assumption that his active experience was conditioned by factors which he has forgotten, is as likely as not to be a projection into the past of valuations and tendencies that are actual at the moment when he reflects about the past. We are continually and spontaneously "interpreting" our memories from the point of view of present active interests. Sometimes we "rationalize" our past by assuming a conscious unifying purpose where there was none; sometimes we "irrationalize" it, by supposing that there was a deeper unconscious tendency underlying the obvious tendencies; sometimes we simply substitute one purpose or tendency for another; sometimes we seem to discover a psychological connection between a nearer and a more distant past about which we knew nothing before. Introspective "self-analysis" (including some of its recent forms current among the adepts of the psychoanalytic school) has developed from this spontaneous practical interpretation of our past; while enlarging its scope, it has not made it any more reliable.

The only way of avoiding, or at least reducing, this retrospective distortion of our active experience is to

distinguish as strictly as possible between *facts* and *explanations*. The facts of our humanistic experience are the values which are given to us and our active treatment of these values. Why these values are given to us as they are, and why we treat them as we do, are not data of our experience, but theoretic inductions concerning the connection between these facts and other facts. Before trying any such induction, we must have the facts clear and be in possession of a reliable scientific method by which connections between facts can be established objectively without any danger of the investigator's personal bias.

For example, when I remember my participation in a certain social group, the facts are the composition and structure of this group as given to me at the time, and the activities I was performing or trying to perform as a member. The problem may arise whether my tendency to participate in this group was not derived from a desire for personal recognition or for domination. I cannot solve this problem merely by probing my past consciousness and attempting to discover these tendencies under my apparent interest in the group. I shall have to get more facts by explicitly remembering other activities in which I manifestly and consciously tried to obtain recognition from other people or to dominate other people. Then I must determine by objective methods whether these activities really were connected with my group activities. To do this, I shall have to use some hypotheses of genetic or causal laws based on previous research and see whether these hypotheses can be applied to my case, or else formulate a new hypothesis which will have to be tested by other experiences and observations. In either eventuality, it will not be a matter of merely remembering forgotten facts, but

of drawing general conclusions from remembered facts.

The other defect of psychological "introspection" by which it has merited the reproach of "subjectivity" is the difficulty of testing its results. This difficulty is entirely of its own making. For the main effort of psychology during the last hundred years has been to reach by introspective analysis the simplest possible "elements" of conscious life—elementary sensations, perceptions, representations, feelings, volitions. Now, it is manifestly impossible to test the results of such an analysis by simply comparing them with the results of somebody else's introspection. Psychology has had to resort to an entirely artificial and devious technique of "experimentation" in which these psychological "elements" are coordinated with parts of a material system created *ad hoc*, which includes organs of the subject's body, certain components of his natural environment and laboratory instruments. The experiences which can be tested by this technique have a very limited range; some of them are artificially produced for the experiment's sake and hardly ever found outside of the laboratory, and few of them are truly significant for the individual's active life. When a psychologist analyzes a really important human experience—social, religious, intellectual, artistic, economic—the psychological "elements" he introspectively reaches evade all laboratory techniques, and it is impossible to control his results.

Whereas original cultural experiences as they actually occur in life can be almost as easily tested as natural observations or physical experiments, their elements are not those rudimentary sensations and feelings which are the product of introspective self-analysis: they are, as we know, objective values which preserve

their content and meaning and are observable by anybody. And these elements do not come in isolation; they are components of actively constructed systems, and (as we have seen) the activities constructing these systems can be formally repeated and functionally shared by anybody. It is, therefore, always possible, at least in principle, to check the description of a personal cultural experience by reconstructing the system which has been experienced, making its composition and structure as near the original as is necessary for scientific purposes. Philology, for example, has been as successful in controlling the objectivity of personal experiences as any science of nature; so have in many cases various branches of the theory of art, particularly musicology. There is no reason why in sociology statements based on the sociologist's reflection about his personal experiences should not be tested by others as efficiently as those of a botanist or chemist.

Of course, by "testing an experience" we do not mean proving or disproving the validity of a general hypothesis by applying it to new instances, but verifying the description of certain data based on somebody else's experience by comparing this description with the results of a theoretic reflection about one's own experience. Such a test can be direct or indirect. The experience of a particular social system can be directly tested only in those cases which permit the reconstruction of the same, and not merely of a similar system, by other people in the same way and from the same point of view as by the sociologist himself. Now, in the social field the range of such systems is perhaps more limited than in most other fields, because many social systems can be reconstructed only by the cooperation of several people with divided functions. Two individuals can, in-

deed, experience personally the same social action, if one reproduces the activity of the other bearing on the same values. But two individuals experience the same social relation from the same point of view only when the relation has been regulated in such an impersonal way as to make it possible for one individual to take the place of the other. The same position, e.g., that of chairman or secretary of a particular group, can indeed be occupied by several individuals; often, however, positions (though similar) are numerically distinct, like those of several physicians or merchants in a community. A group may be experienced in the same way by all its members, if the functions of members have been uniformly schematized; but in many groups functions of members are diversified. Up to a certain point, differences resulting from different viewpoints may be overlooked, as they are even in the natural field; but when it comes, e.g., to a conjugal relation being viewed by husband as against wife, or to a municipality being experienced by a wealthy owner of real estate as against a street-beggar, we obviously cannot ignore the difference.

In many cases, therefore, perhaps in most, the sociologist's experience can be tested only indirectly, i.e., by comparing it with experiences not of the same, but of similar systems. When a sociologist describes an action performed by him, a relation in which he participates or a position he occupies, it is not indispensable that somebody else, in order to test his statement, should repeat the same action, become party to the same relation, or take his position after him, if it can be ascertained that others have experienced or can experience actions, relations or positions of the same kind. But the greater part of the testing in sciences of nature is also

of this indirect type. To test a zoologist's description of an insect, there is no need for others to experience the same individual insect, if they can find insects of the same variety. A laboratory experiment is tested by making experiments of the same specific class in other laboratories.

Of course, the assumption that two systems are of the same variety implies already a theoretic generalization which from a few particular instances concludes about the existence of a class with definite characteristics—and this is a problem we shall treat in our later chapters. A sociologist who describes a particular system which nobody else can experience presumes hypothetically that this system is representative of a class, that there are other systems possessing a fundamentally similar composition and structure; if his hypothesis proves true and other sociologists find similar systems, this experience is indirectly proved (eventually disproved, if these others discover that systems apparently belonging to the same class differ in some important characters from his description).

Obviously, testing the sociologist's personal experience, whether directly or indirectly, is only possible if he describes the systems he experiences in sufficient detail to enable others to identify them or to identify the class to which they belong. How much detail is needed depends on how new or how familiar the particular system or class of systems already is, and for what scientific problem the material described is to be used. The essential condition in every case is simply that the description be made in objective terms (in the sense of cultural, not materialistic objectivity), that is, characterize the values composing the system and the structure of the latter in such a way as to make its re-

construction possible for anybody placed in similar circumstances.

3. *Personal Experiences of the Sociologist: Vicarious*

The specific characters of the cultural world as the object-matter of scientific investigation open to the student a certain source of material which has no parallel in the sciences of nature. A natural system can be either experimentally constructed by the scientist himself, or given to him as existing independently of his interference. The student of culture can also either investigate a system constructed by himself or a system existing independently of his interference. The first case is parallel to the naturalist's experiment, but differs from it in that the personal experience of the student's own activity in constructing a cultural system gives him a specific kind of information which the natural experimenter, being interested only in observing the things and processes included in the system he made, ignores altogether. Now, besides experiencing personally a self-constructed system and observing as an "outsider" a system constructed by others, the student of culture has still a third, intermediary way of approaching the reality he investigates: it is to reconstruct a real system constructed by others, not as a real participant but, so to speak, as an "ideal" participant, dealing with the values involved not as with real objects, but as with "ideas." This we call *vicarious experience*, for it is a substitute for the original experience of the agent who constructed the system.

Vicarious experience is active personal experience. In it the agent reproduces formally and functionally somebody else's activity. But, whereas the original agent

really modifies the values included in the system, the reproducing agent modifies them only "in imagination," that is, acts as if his task were to build a copy of the system in an ideal world instead of reconstructing it in that concrete reality in which his own original activities go on. For example, a student of religion experiences vicariously a religious ceremony as originally performed by a priest when, instead of "realistically" officiating as a priest, speaking the words, making the gestures, handling the implements of cult, praying to a divinity in order to influence it, he reproduces all these performances "mentally" (we prefer to say "ideationally"). He thus identifies himself as agent with the priest, but does not try really to influence the objects of the priest's action, though he recreates in imagination the results of this activity as he thinks these results appear to the priest. This vicarious performance must be distinguished from fictitious performance, as e.g., that of an actor on the theatrical stage, where the original system itself is a conscious fiction which may be vicariously reproduced by spectators.

What makes vicarious experience possible is the fact that many cultural activities are originally "ideational," i.e., deal with values as with "ideas," not as with real objects. Day-dreaming, planning, combining symbols in place of the objects symbolized (the essence of magical action and of much intellectual speculation, metaphysical and mathematical) are examples of ideational activity. The fundamental difference between an ideational and a realistic activity is that the latter uses such *instruments* as are necessary to effect real results, whereas the former does not. Vicarious cultural experience is simply the experience we obtain of a system which originally was or is being constructed realistically

with the help of adequate instruments, when we reconstruct it ideationally in a non-instrumental way.

We are continually using vicarious experience in practical life as a substitute for original experience; and the practical success which often accompanies such a use proves that vicarious experience has a certain validity, that it can give us some reliable information about the systems upon which it bears. For example, in cooperation or struggle with other people we can "put ourselves in their place," by reproducing ideationally their realistic activities, understand or even forestall their results, and adapt our own activities to theirs. An engineer can plan in advance, in every detail, a technical system for other people to realize, even though he has never performed bodily the activities that are necessary for its realization; a statesman can control the political and economic life of his country, even though he has never fully shared most of the activities of its citizens. The success of both engineer and statesman depends on their capacity to "understand," that is, to experience vicariously those activities which they do not know from original experience; if they lack this kind of understanding but are satisfied with the rôle of observers, they are apt to fail.

Vicarious experience has always been used by sociologists; certain schools of sociology have even extolled it as the main, if not the unique source of sociological knowledge. Thus, according to the principle promulgated by Dilthey, the proper method of all cultural sciences is internal synthetic *understanding* as contrasted with external observation and analytic explanation used by the sciences of nature; we live inside the cultural world and mental identification with other cultural agents gives us the only adequate knowledge of their

cultural life. The phenomenological school in sociology, as represented by Scheler, Vierkandt, Litt and others, claims that what we call "ideational reproduction of a system" is a sufficient basis of scientific knowledge, for it gives the sociologist adequate understanding of the meaning of values and insight into their structural connection. For example, Scheler in his book on "Sympathy" has tried to demonstrate the validity of mental reproduction of social phenomena in which we do not participate actively by showing that we can understand feelings which we do not share; a proof of the adequacy of this understanding is the fact that it may lead us to an actual, genuine experience of these feelings.

But the phenomenologists do not take sufficiently into consideration the difference between ideational and realistic activities. When we reconstruct ideationally a system that was originally "ideal," non-instrumental, our reproduction is of the same kind as the original construction, and our experience is self-sufficient. The phenomenological point of view was first applied by Husserl in the field of logic to ideal systems of symbolically organized knowledge; and, of course, to experience fully a logical reasoning we need do nothing else but reproduce it ideationally. When, however, we reproduce ideationally the activity of a husband or wife, a physician or a soldier, a group member or a group—agents who use social instruments to influence other people in real life, our reconstruction of their systems is different from their construction, and our experience obviously differs from theirs. The question is, how relevant are the differences for scientific purposes.

This question cannot be answered *a priori* for all cases. It depends on the theoretic problem for which the given material is to be used and also on the sociolo-

gist's ability to "understand," to obtain vicarious experience approaching original experience on the scientifically relevant points: some sociologists get much better results from vicarious experiences than others. There must therefore be some tests of the reliability of such experiences. The history of sociology is full of worthless generalizations based on inadequate ideational reproductions of social systems. We need only recall as typical instances most of the theorizing about "savage life" and "savage psychology" from the middle of the eighteenth to the end of the nineteenth century, or the nonsense most travellers still continue to write about the civilized nations which differ from their own, whenever they try to interpret "intuitively" certain social systems of these nations.

In order to test effectively a vicarious experience, we must not only compare it with an original experience, but also supplement it by observation to be sure that it refers to the same system or the same kind of system as the original experience with which we compare it. For instance, my vicarious experience of the woman's side in a conjugal relation, of the social rôle of a statesman, or of the structure of a parliament can be proved valid only by comparison with the actual experiences of married women, of statesmen when functioning in office, of active members of parliament. Hence, to enable others to make this comparison I must describe my objective observations, as an outsider, of women as wives, of statesmen, of parliaments, so as to make it possible for any observer to find the kind of conjugal relations, official positions, parliamentary groups I am vicariously experiencing among the systems which other people actually construct. Otherwise my vicarious experience may be fictitious; I may be describing a kind of

system which does not exist outside of my imagination. Nobody would be able to prove or disprove whether my description corresponds to facts if I give no observable data by which to identify the reality I have in mind.

We may thus formulate as a general rule that, while a description of an original experience must be in such terms as to be verifiable by other original experiences, the description of a vicarious experience must be made in such a way as to be verifiable both by original experience and by observation. It is significant that sociologists who excel in ideational reproduction are usually the ones who feel most the need of testing their vicarious experiences. Thus, William I. Thomas, whose capacity for vicarious experience is probably unrivalled in modern sociology, has made a wider use than anybody before him of observable data and documents describing original experiences of other people.

4. Observation by the Sociologist

Since all culture is given to the student with the humanistic coefficient, whereas nature has no such coefficient, the observation of cultural data must be obviously different from the observation of natural data. But the difference is not such as to preclude the possibility of attaining the same degree of scientific objectivity in both cases, though in both cases it is perfectly obvious that observation implies some kind of personal experience, and the student would know nothing theoretically of reality, if he had not had practical contact with a reality of the same or a similar kind.

In both cases the observer must start with primary sensual data, and in both he must go beyond them, in-

terpret them as significant of a certain objective reality, though a reality in one case is regarded as independent of man, a world of things connected into systems by natural forces, in the other case as existing for man and through him, a world of values connected into systems by active human tendencies. For instance, we interpret a certain combination of colored spots among other colored spots as a stone or a tree; another combination as a church or a letter; a certain spot changing position as a moving ball made of rubber; another spot changing position as a player trying to catch it. We have to learn both natural and cultural interpretation gradually, by innumerable observations continued from the day of our birth. And we do learn it by dealing actively with observable data. We realize the solidity and weight of material bodies owing to their resistance to pressures actively exercised by us with the help of our own body; we understand what movement is, because we move our body in its environment and modify the position of things in this environment; we are aware what a church is, because we have participated in worship; we know that a letter has a social meaning, because we know how to write and to read; we realize what the player is doing when he tries to catch a ball, because we have caught more than one moving object.

The difference between things and cultural values, as far as methods of observation are concerned, means simply that in observing natural things we need to take into account only what these things signify in our experience as we have learned to interpret them (or similar things) in our past, whereas in observing values it is indispensable to notice how other human agents deal with them, and interpret this treatment in the light of instances where we actively shared the experience of

these or similar values with other agents. Sometimes the distinction is learned very late in life; many people, indeed, never succeed in drawing it sharply enough for purposes of scientific observation. They interpret natural things as values by assuming implicitly or even explicitly invisible conscious agents dealing with them; or, oftener still, they treat values as things and believe that certain characters which these values possess only in particular cultural conditions (e.g., economic utility or religious sacredness) belong to them absolutely, are inherent in their "nature."

A scientist who in observing a cultural activity refers it to the world of things commits thus an analogous error of method as a scientific observer of natural processes would commit if he interpreted them as going on within the world of values. The latter kind of error has been almost eliminated since the Middle Ages: no scientist nowadays would think of defining the nature of an earthquake or of an epidemic by its use as an instrument to punish men for their sins or to foster human progress. But the parallel error in cultural interpretation is still very frequent. Thus behaviorism, explicitly devised as a method of observation of organisms as natural things responding to the stimuli of their natural environment, has been used to interpret the conduct of conscious agents dealing with cultural values—linguistic symbols and technical instruments; the human geographer often views the technical and economic activities of men as causally conditioned by their natural milieu. We shall be forced more than once to point out theoretic consequences of such errors of method; at this moment we wish merely to emphasize the fundamental principle that in sociology, as in every other cultural field, the observer of cultural systems independent

of himself must use the humanistic coefficient and realize that he is observing values and activities, just as he is experiencing values and activities when he constructs a system.

Thus, take the ethnologist who visits an unknown tribe. He is faced by numerous and varied cultural values: material objects showing that they have been handled by man, words of an unfamiliar language, dances and other obviously ritualistic performances, songs, pieces of instrumental music, etc. He knows that these are cultural values from his own past experience, but he is also aware that they are different from the values he knows, that he does not understand their meanings and cannot reconstruct the systems to which they belong. If he should have failed to become aware of this at once, he may be unpleasantly forced to realize it by having his attempts to use these values in his own way actively interfered with by the tribe. And this is, indeed, a particular illustration of the objectivity of values: we find that objects which are values to us are also values to other people, even before social communication is established through speech, because their activity modifies the content and meaning of these values for us, and because they react favorably or unfavorably to the axiological significance we give these values in our activity.

As long, however, as the ethnologist merely looks and listens, eventually even handles certain values of the foreign culture in his own way, he is not yet observing this culture scientifically. Scientific cultural observation begins at the moment when he attempts to study theoretically the various systems into which the given values are combined in the cultural experience and activity of the tribe which he investigates; and then

he learns to know at the same time the meaning of these values and the tendencies involved in the cultural life of the tribe. And the only way this can be achieved is by investigating the ways in which the given values are dealt with by the members of the tribe.

When the ethnologist notices a piece of metal strangely shaped, he can ascertain what this piece means as a cultural value only by observing how it is handled, in connection with what other objects it is used and what the methods are of using it. It may prove then to be a technical instrument, technical material to be remodeled, a weapon of war, an implement of religious worship, or a work of art; in any case, its meaning will be understood only by finding the rôle it plays in a cultural system, and the system as a whole must be studied simultaneously. The same is obviously true of a word of language which cannot be properly understood except in connection with the object symbolized on the one hand, with other words in sentences on the other hand; or of a ritualistic gesture, a formula, a mythical personality, whose meaning is incomprehensible outside of the religious system to which they belong.

In no other field is this need of observing whole systems of values as manifest as in the sociological field. Social values are men. To the ethnologist who comes to study an unknown tribe the individual members of the tribe and the tribe itself are social values in so far as he attempts to influence them into giving him information and thus gets into practical contact with them, is admitted into their intimacy, or else sees himself threatened and has to fight or escape. He may be, moreover, a missionary and try to convert them, or a merchant and try to induce them to trade with him.

In these ways he obtains some personal experience; but it is personal experience of his own social values and tendencies, not of those of the tribe or of its members. He experiences indeed their reactions, but these reactions merely characterize them as they appear to him while they are objects of his own actions, and are worthless to characterize them as human beings or social agents in general. Nor are any observations he can make while acting upon them of much value; at best, if he is very impartial and thorough, he can learn something about their way of treating strangers of the class to which he belongs in their eyes. And yet, at least nine-tenths of the older ethnological literature, and much of what is being published now, contain little more about the social life of other peoples than the traveller's or missionary's experiences of his own social behavior.

Scientific sociological observation requires as its first condition that members of the foreign tribe and the tribe itself be viewed as social values for one another. What does each individual mean socially to each other individual? What does the group mean to its members individually? What does a member mean to the group collectively? If there are more groups, what do they mean to one another? These are some of the first problems a sociological observer encounters. And he cannot solve them except by studying how individuals and groups actively treat one another, and thus reconstructing theoretically step by step the habitual social actions, the normatively regulated social relations, the personalities and the groups to be found in the tribe.

Now, this kind of theoretic reconstruction of a system which is involved in all cultural (as well as all natural) observation is also present, as we have seen before, when we wish to utilize scientifically our own per-

sonal experience of a system. It is a theoretic activity which in the latter case is superimposed over the practical activity of constructing a system, realistically or ideationally, and must be sharply distinguished from this reconstruction itself. Sociologists as a rule do not make this distinction; thence the common error of the psychological school that all sociological observation implies mental reproduction or that, as Eisler expresses it, "Sociology cannot help presupposing, for the explanation of social becoming, those forces which the sociologist finds immediately only in himself," and it needs therefore the principle which Wundt has called the "principle of subjective judgment" (*Prinzip der Subjektiven Beurteilung*).

The point is that active reproduction of systems may go on, and is continually going on, without theoretic observation of these systems preceding, accompanying or following it; and *vice versa*, theoretic observation of systems may go on without being preceded, accompanied or followed by active reproduction. The former statement holds true not only in cases in which the agent reconstructs familiar systems, but also in most of those in which he constructs systems for the first time himself, although they may be already existing in his cultural milieu. In learning to speak a foreign language, to use an unfamiliar technique, to occupy a new personal position, to participate in a group into which he is received, the individual seldom begins by reconstructing these systems theoretically. The usual way, the one used by children and uneducated people, is the one of trial-and-error, with or without educational assistance. The naïve individual faced by unknown values presented in an unfamiliar way begins by acting as he used to act in somewhat similar circumstances. But among foreigners,

he expresses his needs in his own language, accompanied by gestures, applies to unknown materials and instruments some of the technical methods he knows from previous experience, behaves in his new social position as he used to behave in some previous position, treats his fellow-members and the institutions of his new group more or less as he did those of groups to which he has earlier belonged. Then as under the pressure of external influences, his system fails to be achieved, he modifies it, until after a number of trials it becomes identified with or similar to the new system. There is all the time personal experience of the unfamiliar values, of the system he is constructing, of the interfering influences, but very little observation, if any. The child does not spontaneously learn a foreign language by listening to foreigners, analyzing their sentences into words and then reproducing these sentences, but by substituting in his own speech such foreign words and whole short sentences as he has grasped, instead of native words or gestures. The newly elected group leader or the recently appointed salesman does not "naturally imitate" the behavior of former leaders or other salesmen, but tries to do what in the light of his previous experiences he thinks he should be doing. We fail to realize the whole extent of the trial-and-error adaptation of old systems to new conditions, because the individual makes many of his trials "mentally," reproduces a familiar system ideationally and, if it does not seem to fit the new conditions, modifies it also ideationally before attempting instrumental realization. Often also the individual is consciously assisted by the educational activities of others who shorten his trial-and-error adaptation, helping him get practically acquainted with unfamiliar systems.

Of course, active reproduction may be preceded by theoretic observation. Learning a language in school differs from practical learning precisely in that the learner is made first to observe how the language is spoken or written before speaking or writing it: he analyzes a sentence into words, a word into etymological elements, studies their connection, and only then reproduces it. But it is a well-known fact that practical use of foreign languages is not learned in school: observation is not a substitute for personal experience. On the other hand, theoretic knowledge gained from mere observation, though not as satisfactory as when it is based on observation and personal experience combined, can still be quite reliable. A foreign philologist may never have the chance to learn a language practically, and yet know more about it than even an intelligent native who relies only on his own experience and has never carried on scientific observation.

In truth, not only is active reconstruction of a system not a necessary prerequisite of its observation, but precisely where reconstruction fails observation comes in as a source of knowledge. We can reproduce realistically or ideationally only systems which are not too different from those we have already experienced: even with educational assistance, the field of our personal experience can widen only gradually. When the sociologist is facing an unfamiliar system very different from the ones in which he has participated, theoretic observation is the only reliable approach. Until he has investigated it thoroughly from the outside as a non-participant, it is better not to try to get an "inside" experience, for he is apt to look for familiar meanings and relationships and to miss the very features which make the system different from those he knows. The history of ethnol-

ogy is full of such mistakes. Even real active participation in foreign social life does not always insure against them, for the individual participant may only grasp superficially certain significant foreign values, while their deeper meaning eludes him.[1]

In addition to facts directly given to the sociologist who is witnessing social activities of other people, there is another kind of objectively observable social data connected with the use of written language as an instrument of social activity. Nearly all social activities use words, mostly in combination with other instruments, to influence the behavior of other people. An outsider, listening to words used in this instrumental significance and understanding their meaning, is obviously making objective social observations just as well as when he is looking upon outward movements of the body and realizing their instrumental function. There are few social tendencies whose active manifestation is complete without verbal instruments, whereas there are many which need no other instruments but words. Now, verbal activities on higher levels of culture are often performed in writing or even in print, and the written document remains an observable datum giving to anybody who knows the meaning of the written symbols a direct grasp of the values involved and their structural

[1] I have often been struck, for instance, by the stunted and superficial conception American-born citizens of foreign-born parents have of the most important standards of American social and even political life, particularly when their parents belonged to the working-class and lived in an immigrant community. Active participation there is, but reduced to secondary-group contacts, since immigrants are not admitted into intimate relationships with natives; and since secondary-group norms have grown, and still remain in some measure founded on primary-group relations, it is impossible to understand the former without knowing the latter.

relationship to the given social system. This extends the field of sociological observation far beyond those social systems whose human elements can be sensually given to the observer. Of course, as everybody knows, documents seldom, if ever, furnish complete direct data about any particular social system, since all the non-verbal instruments and the verbal instruments not symbolized in writing are absent from the documents; but as far as the documents go, if they are authentic and the meaning of the written symbols clear, no doubts can exist as to their reliability as objective data. For scientific use they may be often more valuable than those which the sociologist finds when observing an activity in the very course of its performance, for they can be investigated at leisure and, being accessible to all, their individual interpretation can be immediately controlled by others.

Here belong such materials as correspondence between private individuals, group members and functionaries, or functionaries representing different groups: legal enactments, administrative and military orders, judicial decisions; exhortations and preachings of moralists, press campaigns, advertising and propaganda; spontaneous or experimentally provoked expressions of desires, ideals, valuations as substitutes for full social action and aiming to influence verbally the social objects to whom they refer.

All such documents can be safely taken at their face value, provided the sociologist does not presume to conclude from them about anything but the very activities which they express—though he is often tempted to do so. There are, indeed, three important dangers to be avoided in interpreting such data. First, it is always unsafe to presume that a verbally expressed activity has

provoked the reaction it aimed to provoke, unless we have either direct evidence in the form of other documents expressing this reaction or a sufficiently substantiated indirect evidence of a positive kind. This is a very important point where written rules, particularly state laws, are concerned. A legal document is a first-hand sociological material, but only when viewed as a trace of the legislator's activity; and nothing can be inferred from it but the intention of this activity. What reactions it actually provokes on the part of those to whom the legislator means the law to apply is an entirely different problem which needs other specific data to be solved. Indirectly, as we have seen before, this document may serve to draw certain conclusions as to the norms recognized in the given social group, but only in so far as it can be shown to formulate norms which have been already prevalently applied by members of this group.

A second important danger in utilizing documents of this class results from the lack of clear distinctions between the document as an instrument of action and the same document as a source of information. A court decision, a public or private letter, a moral invective, a newspaper article, a personal appreciation, or an autobiographic self-expression usually contains statements of facts, descriptions of the behavior of other people or of the author's own activities. Now, these are obviously not first-hand data for sociological observation, but alleged truths concerning the experiences and observations of the author of the document, who in stating them plays consciously or unconsciously the rôle not of social agent, but of observer. How far his statements can be relied upon and utilized is a problem to be investigated presently; anyway, it is entirely different from the problem of utilizing his document as a mani-

festation of his active tendencies, intentions, or tasks. We know directly by perusing a court decision, a declaration of war, a begging letter, a proposal of marriage, a sermon, an article of propaganda, what the author was doing when he wrote this document and communicated it to the recipients; for the social object, the intended result, the ways and means by which the result was expected to be attained are manifested by the content of the document. But we do not know whether, for instance, any of the theoretic arguments he alleges to support his action are true or false.

Often, indeed, it is possible to utilize as direct evidence even statements of fact, if these are taken without any regard to their truth or falsity, not as theoretic views, but merely as expressions of the author's practical social attitudes. For usually people who are not trained in purely theoretic observation color their statements of fact axiologically, use terms of positive or negative valuation. Now, every valuation, whether the object valued is or is not objectively such as the author claims it to be, gives evidence of what the author considers it should be; and verbal expressions of normative views are really potential social activities aiming to impose the norms expressed upon human behavior.

A third danger in dealing with documents as first-hand social data comes from a common inclination to treat the intentions or ideals manifested in such documents as being "sincere" or "insincere," indications of more or less permanent active "dispositions" of the individual or the group from whom the document emanates. In a diplomatic correspondence intentions are often expressed which, because they conflict with other, hidden intentions, are never meant to be realized otherwise than in words; many a preacher or politician ex-

presses in public ideals which in his private activities yield to opposite purposes; private letters often contain expressions of affection or admiration which are "insincere," that is, which conflict with stronger opposing tendencies not manifested openly by the author; an autobiographer wishing to influence his readers favorably formulates sublime standards and valuations which have never markedly affected his activity.

It would be an obvious and familiar mistake to base upon these verbal manifestations any conclusions concerning their author's judgments as to the "real inclination," "trend," "disposition," "social nature" of the group or the person concerned. But it is an equal, though less obvious mistake to reject this kind of data altogether on the ground that they do not permit such conclusions. The verbal expression is a social action, and though we cannot judge from it what other social actions the agent does or does not perform, it constitutes a datum in itself; the tendency, intention, ideal, standard expressed actually exists, though as a social force it may be barely strong enough to manifest itself in words and may yield to other, stronger forces whenever it conflicts with them. The friendly intentions expressed by a diplomatist in the name of his state are actually significant, at least while and in so far as the diplomatist is entitled to act for his state; their expression is a friendly act, though it may be of very little importance as compared with other acts simultaneously performed or projected. The politician's ideals expressed from the platform are real in so far at least as their expression is meant to affect the public at the time of the meeting, although the politician after stepping off the platform may never again actualize them in his behavior. Many of the elevated sentiments

expressed by Rousseau in his autobiography are no direct indications of his character, but their expression is a *prima facie* evidence of their existence as social forces sufficient to bring forth this very action of expressing them for the benefit of the reader.

There is a gradation leading from social activities performed in full and producing in social reality lasting and generally observable modifications, like the activities of Napoleon and Washington, down to such stunted activities as bring no appreciable direct results, like the day-dreaming of a boy who imagines himself in the rôle of a Napoleon or a Washington; and verbal expressions may be scattered on various grades of this scale between the two extreme limits. The sociologist who wants to be careful, and yet does not like to waste good material, must learn how to extract from a document all the data it contains, while he avoids ascribing to these data more real importance than they actually possess.[1]

5. *Personal Experiences of Other People*

Second-hand evidence is accessible to the sociologist through the medium of verbal or written symbolic expression. Two points must always be kept in mind while

[1] The distinction we are making here between language as a social and as a non-social phenomenon is different from the conception of Kimball Young, who views language as essentially social, but distinguishes between its objective or rational and its subjective or emotional use. We do not deny this distinction, but claim that it is secondary, the primary difference being between social and non-social uses of language. The social use is instrumental and manifests social tendencies, whether the words have a prevalently rational or a prevalently emotional significance. The non-social use is not instrumental: verbal systems are self-sufficient, whether they symbolize rational relationships as in logic, or sentiments as in lyrical poetry.

utilizing this kind of material. First, that in the very measure in which spoken or written words are valuable as first-hand data for scientific observation, they are worthless as a second-hand source of theoretic information; secondly, that the reliability of information intended as theoretic depends on the scientific training of the person who imparts this information.

The first point is very important for the sociologist because precisely in his field many, perhaps most, verbal statements combine the character of social instruments with that of symbolic expression of the subject's experiences and observations. On the one hand, we have seen that statements of fact often involve valuations which are manifestations of practical attitudes, and as such constitute observable sociological data; in some cases, as in clever political propaganda, it may be impossible to eliminate these from a seemingly theoretic statement without some additional source of information.

On the other hand, speech or writing openly intended as verbal social activity usually contains theoretic information which can be used as reliable second-hand material: this is mostly unintentional information which the agent did not mean to impart, but implicitly assumed as already familiar to the people he intended to influence, accepted as the common "universe of discourse" within which his verbal activities were to go on. For instance, though the writer of a begging letter may purposely misinform the recipient about his own situation so as to provoke his sympathy, if he mentions incidentally some situation common to both of them (say, their past friendship), we can safely assume that he speaks the truth as far as he knows it and is capable of being objective, since any deviation from truth would

be detected by the recipient and would interfere with the purpose of the petition. While the political agitator usually distorts the facts he supposes unknown to his listeners, he cannot risk misinformation when he quotes facts presumably known to them. In short, wherever in a verbal activity the agent utilizes experiences and observations common to him and to the person or group he means to influence, the information his words contain is as reliable as he can make it, since it is subject to immediate control of this person or group, and would miss its purpose if it did not stand the test. Much valuable data may be thus gleaned at second-hand from the study of verbal activities, oral or written. Historians know it well and have developed a highly efficient technique in this line, particularly when dealing with periods about which documentary evidence is scarce.

However, this source can only offer fragmentary information, for references to familiar facts are naturally apt to be brief and incomplete; the social agent bent on practical results is not inclined to describe at large data he presumes already known to his social object. The main sources of second-hand sociological material must be descriptions made by authors for the very purpose of imparting theoretic information to others. And here from the point of view of reliability the distinction between descriptions of the author's own active social experiences and descriptions of his observations of other people's social performances becomes important. The two kinds meet whenever the author describes his interactions with other people—e.g., social relations to which he was a party.

The earliest purely theoretic descriptions of the author's own active experiences were probably made

in response to social demands for objective information concerning activities which other members of a group did not share. Thus, we find the old and experienced in war, politics and private intercourse communicating their experiences to the young, those returning from a war or mission relating their deeds to those who stayed at home, those in authority rendering account of their political deeds to the mass of group members or to posterity; later, whole groups reporting their activities to larger groups of which they are subordinate parts or to the public at large. This is how autobiographies of individuals and auto-histories of groups originated. While seldom devoid of all practical purposes of self-glorifi- ion and self-justification, such documents nevertheless cannot be considered entirely invalid, since the social demand to which they respond is usually accompanied by some pressure exercised in favor of truth. Those who ask for information about active experiences in which they did not participate insist upon the information's being reliable (unless, of course, they share the practical aim of the author, as in the case of group members who wish to glory in or do not wish to be ashamed of the actions of their ancestors, chiefs, warriors or ambassadors). The strength of this insistence and the possibility of controlling the assertions of the author determine prevalently the relative reliability of the document.

The stress upon truthfulness is, of course, particularly high when communications concerning personal experiences are in advance destined to be used as material for theoretic purposes. Historians and biographers have opened the way here by asking active participants in certain events to relate their experiences with the explicit purpose of utilizing them for synthetic descrip-

tions of these events. Later, ethnologists did the same when wishing to ascertain the details of certain practices, though most of them have dispensed with such concrete material and been satisfied to supplement their own observations with abstract statements given by their interlocutors about tribal mores and beliefs.

Finally, modern sociologists came into the field and began to gather vast masses of this material under scientific control. Descriptions of other people's personal experiences may be collected with or without reference to definite sociological problems formulated in advance. In the former case, personal interviews and written questionnaires are employed; in the latter, the subjects are induced to write complete autobiographies or histories of the groups of which they have been active members. The technique of the interview has been elaborated very thoroughly in recent writings by American social workers and sociologists. The interview has the obvious advantage of allowing the student to fill out the gaps of spontaneous information by adroit questioning; on the other hand, the interview itself is a social experience for the person interviewed which often is practically important in his eyes; sometimes it affects the reproduction and description of the experiences about which he is interviewed, leading him to distort the original facts.

The questionnaire has much less of this disturbing influence; moreover, it is easier to plan in such a way as to discount in advance the disturbances which various social motives may introduce into the written answer. On the other hand, it is difficult to supplement the deficiencies of the answers. The best thing would be to have a written questionnaire followed by a personal interview. And I should like at this point to put the

strongest possible emphasis on the distinction between two types of questionnaires: one in which the purpose is to have every answer give as rich and detailed a description of particular experiences as possible; the other, which aims only to learn to which one of pre-established classes the author's experience belongs. The latter, typically requesting answers in the form of "yes" or "no" (or a mere check) is only valuable for statistical tabulation, and should be limited to those problems which can be statistically solved. But this is a question belonging to our next chapter.

The autobiography written to order is, in my opinion, at the present stage of research the best kind of second-hand source for the study of active social experiences, provided it is sufficiently comprehensive to give insight into the details of personal activities. Of course, we cannot determine in advance on what problems such a document will throw light, for this depends not only on the kind of social experiences the author has had, but also on the standards consciously or unconsciously used by him in selecting for description certain experiences rather than others. On the other hand, there is no bias imposed upon him in advance, as in the case of the interview and the questionnaire; he is free to describe whatever he is spontaneously inclined to.[1]

To guarantee the "truthfulness" of the document, the same means must be planfully used which society has always used unreflectively for this end. First, truth must be explicitly demanded and a premium set upon it. I have been doing this by offering pecuniary and

[1] I have found after many trials that, roughly speaking, 100,000 words (about 300 typewritten pages) is the *minimum* size an autobiography should possess to be really valuable as a source of sociological information about personal active experiences.

honorary rewards for the best personal documents, emphasizing thoroughness and truthfulness as the standards by which these documents were to be judged. With a higher type of person, the ideal of social service, the conviction that on the truthfulness of the document depends its utility for the advancement of science and for its practical application, has often proved a powerful incentive.

Secondly, a way must be found of controlling by other testimony, in some measure at least, the statements obtained by interview, questionnaire or autobiography. Of course, complete control is seldom possible, but most statements of personal active experiences can be tested with an approximation sufficient to make them valuable. There is the eternal evidence of veracity obtained by comparing various statements of the same person. In this, a comprehensive autobiography has the advantage over the interview and the questionnaire. When an active experience is shared by several persons, their separate statements can be collected and then compared and checked. This is really the best method of assuring both completeness and veracity of description, though as yet not very much developed in sociological research. Its main field of application is, of course, the study of group-life, and I believe it has a great future in this field. By asking a number of group members, each separately to detail personal active experiences connected with his participation in the group, we obtain the best evidence concerning the actual reality of the group as a social system, in which evidence the various personal statements partly control, partly supplement one another.

6. Observations by Other People

As compared with other people's own active experiences, observations made by them as outsiders have only a limited importance as a source of sociological material. Anybody with a sufficient mastery of a language and willingness to tell the truth can communicate his own active experiences. Though he may emphasize such features as the sociologist in his place would consider unimportant, and omit such as the sociologist in his place would emphasize; though he may sometimes ascribe to values a content and a meaning different from what the sociologist thinks they possess; this is as it should be, since it is his own experience and not the sociologist's he describes. Whereas to observe and describe adequately systems in which one does not participate, one must be free from one's own practical bias, realize the difference between one's own practical point of view and that of others, emphasize objects and facts which are important to others, even though they may seem unimportant to the observer in his function as social agent, take into account such characters as others ascribe to objects and facts, even if to himself these characters would seem illusory. This can be done only by an observer who, while intellectually interested in other people's social behavior, has excluded his social interests from the field of his observation.

Nearly everybody has a certain amount of intellectual interest in social life free of practical considerations, but most people lack the training necessary for a thorough observation. Even while merely observing the behavior of others with no reference to their own social pursuits, they still unconsciously interpret it in terms

of their own habits, norms, beliefs or prejudices. This does no harm, if the habits, norms, beliefs and prejudices of the observed are the same as those of the observer; but it obviously deprives the description of theoretic validity when they are different. Such limitations are important when dealing with the enormous mass of descriptive material contained in historical chronicles, accounts, biographies, memoirs. Historians and ethnographers, indeed, are often forced to use this kind of description when they have no better; but the sociologist, who is not bound to reconstruct particular periods or peoples and can find similar, but more reliable data elsewhere, need not strain this evidence.

To observe and describe adequately social activities widely different from those one has been used to perform, special training must be added to intellectual interest. Like every training, it is best achieved in connection with professional pursuits. Now, there are two kinds of professional observers of social life: men of letters and scientists.

The problem of the validity of literature as a source of sociological information has been raised and discussed only quite recently by sociologists.[1] There is a marked conflict between two points of view: that of the radical rationalist who would like to make sociology independent of any data which are tainted with "subjectivism" and cannot be made absolutely impersonal; and that of the intuitionist who stands for the theoretic importance of a "direct" knowledge of reality, treats rationalization as worthless abstract schematism, and considers a glimpse of genius as worth more than all the careful plodding of pedants. The methodologist

[1] The first systematic attempt at evaluating literary contributions has been undertaken by *Sociology and Social Research.*

who has no prejudices either way, but wishes to exploit as far as can be done every source of possible knowledge, must deprecate both these radical views concerning the validity of artistic observation. Against the rationalist he must appeal to the empirical origin of knowledge. Personal experience and observation are the ultimate bases of all knowledge, the final criteria of validity of all general concepts and laws. And undoubtedly there is as much good observation and careful description of social data involved in works of literature as in works with scientific claims. In barring entirely the use of literature for scientific purposes we would certainly deprive ourselves of a valuable source of material.

And yet if we use this material for scientific purposes, we have to be very careful. Against the intuitionist, we must remember that the scientific importance of a particular case does not lie in itself, but in its use for the formation of general concepts and laws. A case well observed is scientifically important in the very measure in which it is representative of a whole class of cases which have not been so well observed; and its description is valuable in so far as it is valid for the other cases of this class, and thus dispenses us from the need of studying them.

Now, the man of letters claims precisely that his cases are typical and that every one of his descriptions is valid for a whole class of data. And, indeed, his very aim is to select representative cases and in characterizing them to emphasize features which with individual variations are common to all the cases they represent. The only trouble from the point of view of the scientist is that the man of letters does not substantiate his claim theoretically, does not subject his description to such a

test as would demonstrate explicitly how, and in what respects, it is valid of a whole class. He cannot do it because his description, though typical, is not schematic; he treats his case as a concrete individual case, and does not separate its particular from its general features, but expects his readers to see for themselves how far his case is representative of other individual cases they know, and how far it is unique. Therefore, he does not hesitate to construct an imaginary case that does not correspond to any real case in particular for its individual detail, even though it is supposed to correspond to real individual cases in its typical features. The scientist who wishes to rely entirely on literary cases as evidence would find himself in a position where he would be forced to accept the artist's view of what is typical, essential to a class of cases, as if this view were the result of scientific induction, without being able to test the method used in this induction.

Consequently, literary evidence can be utilized only as auxiliary evidence, but in this character it may indeed be useful. The scientist has no right to accept the artist's presentation as an inductive basis for any generalization in the same way as cases observed by himself or by another scientist, but he may use this presentation as a help in his own induction. The fact is that in the social field the artist, particularly the dramatist and the novelist, has often played the part of a pioneer who opens up new domains for observation. The sociologist who follows him may find his trail uncertain, irregular, dangerous, and have to substitute a straighter, broader and safer road instead; nevertheless, the earlier landmarks may be of the greatest service for provisional orientation. Many a sociological problem has already been, and many more will yet be suggested by works of

art, for the literary genius often sees first the importance of facts neglected and ignored by the theorist pursuing his doctrine. And while testing a sociological hypothesis, in addition to data scientifically observed, it may often be well to take into account literary descriptions bearing on the matter, not so much to gain any new certainty from their agreement with the hypothesis as to see whether they do not throw any side-light which would raise new problems.[1]

No such restrictions attach, of course, to the use at second-hand of observations made for theoretic purposes by scientifically trained observers, who intend and know how to comply with the usual scientific standards of thoroughness and objectivity. We spoke above of the conditions with which a sociologist as a scientific observer must comply to make his observations fully valid. In so far as a scientist uses factual evidence gathered by other scientists, he is of course concerned about the reliability of this evidence. But he is at least as much interested in the theoretic significance which the facts observed by others have for the solution of his problems. In a well organized science with an effective and many-sided intellectual cooperation, this mutual interest of scientists in each other's data leads not only to a high level of reliability in published observations, but also to the development of standards of selection by

[1] Professor W. Kilpatrick in a private conversation attracted the present author's attention to the important part the *consensus of critical opinion* has in enhancing the sociological validity of a literary document. Indeed, if well-informed critics agree that a fictitious case shown in a literary work possesses characters typical of a certain class, their judgment becomes almost equivalent to the confirmation of a generalization by new experiences and observations, since we can presume that it is based on a comparison of the fictitious case with real cases with which they are familiar.

which theoretically significant data are distinguished from those which an individual student may wish to observe for reasons of personal curiosity, artistic sense or pedantic absorption in detail, but which are of little importance for the advancement of scientific theory. Unfortunately, sociology has not reached yet this stage. Many sociologists choose their observations as a collector selects "curios" or a novelist matters of "human interest"; or else consider themselves in duty bound to get the most exhaustive information about some concrete fragment of social reality—a tribe, a rural or urban community, a racial, national, religious group—without any preconception as to the theoretic use to which such material can be put. Many others again search only for data to prove some doctrine of their own, and consequently their observations are useless to every one except those who agree with this doctrine. The distinction between significant and insignificant observations can come only with the growth and systematization of sociological theory; as in older sciences, problems suggested by existing hypotheses will guide the student's choice of data to be observed.

7. *Generalizations Used as Materials*

In sciences of nature all general statements are theories; between the description of a particular chemical process or a particular plant and the formulation of the general law of a certain kind of chemical process or the characterization of a whole species of plants, the distinction is sharp and unmistakable. Whereas in sociology the matter is not so simple, for there are many human generalizations bearing on social data which the sociologist must treat *as data* to be studied, not as

theories to be accepted or criticized in connection with his own theory. We find two types of such generalization.

First, in social life individuals and groups as objects of social activity are classified, and such classifications are significant as indicating real similarities and differences of social behavior with reference to them. If we find, for instance, among the members of a religious group, a general statement current that the members of a rival religious group are "priest-ridden" and "superstitious"; or if the division of the population of a territory into "nobles" and "villeins" is accompanied by the view that this division corresponds to differences of racial origin, such a general statement or view is of course not to be taken (at least, not without being tested by methodical research) as a theoretic truth concerning the rival religious group or social classes, such as they really are. Yet it is a sufficient indication that among the members of the first religious group there is a *general tendency* to treat any representatives of the rival denomination as priest-ridden and superstitious, and that among the population of the given territory, the nobles are generally treated as if they were of different racial origin than the villeins.

Another instance: in all societies, high or low, there are numerous psychological concepts current which serve to characterize individuals by subdividing them into psychological types—"good," "bad," "wise," "foolish," "clever," "stupid," "courageous," "cowardly," "generous" or "mean." The content of these concepts varies and changes; from the theoretic point of view they are usually worthless. But to the sociologist they are important as practically real social data, in so far as they indicate: first, how at a given period and

in a given collectivity social individuals in general are regarded by their social milieu, what possible typical reactions this milieu expects and prepares for when acting upon them; secondly, what the current aims of social education are in this collectivity, for education always purposes to develop desirable, and to repress the evolution of undesirable characteristics.

A different kind of significant social generalization is involved in collectively recognized *norms* regulating social behavior. A norm by its very nature is at least implicitly "general" in that it demands actions of a certain kind, whenever and wherever people are bound by a certain social connection; and thus it may be said to apply to an indefinite number of particular actions. Therefore when a norm finds reflective expression, the latter includes general concepts; the whole domain of law is constituted by such reflective expressions of norms. These normative generalizations have no direct theoretic validity; but, besides being verbal instruments of action in certain groups,[1] they are also indications of a practically general state of things, i.e., they show that among the members of the group which endorses the abstract normative formula there generally exists the intention to behave and to make others behave in certain relations according to the principles expressed in this formula. Of course, this intention in particular cases may be counteracted by other factors, and therefore the legal formula does not permit us to draw the conclusion that all the members of the group actually do behave always in the way formulated—there would be no need of any legal activity, if they did; but merely that many of them actually intend to do it, seriously or lightly, determinedly or vaguely.

[1] Cf. Chap. III, secs. 5 and 8; Chap. IV, sec. 4.

This conclusion may be drawn not only from legal formulas, but from other general expressions of norms, like those found in various kinds of literature—not because in such a case we rely on the generalization of the author as upon a theory of social facts, but because we trust that the practical attitude expressed in his generalization is actually one he has found generally accepted in his milieu. Hence the very fact of his expressing it subjects it to the test of public approval, for he knows that his work will not be well received if it expresses norms disagreeing with those actually and generally recognized. Of course, he may have put upon these norms an emphasis altogether out of proportion to their relative importance in real life, acting, e.g., under the influence of a professional bias,[1] but this is a matter of degree to be adjusted in each case with the help of other evidence, particularly facts showing transgression of norms.

The sociologist can thus use other people's generalizations as material (and first-hand material, at that) in so far as a generalization may be presumed to play a practical rôle: 1) to express a feature which people rightly or wrongly assume to be common to certain values and take into consideration when dealing with those values; 2) to express a rule or principle which people regard as binding for many social systems and in accordance with which they try to shape these systems. The significance of this material depends not upon the theoretic adequacy of the generalizations with reference

[1] Thus, among mediæval authors, those writing poems to be read in court circles overemphasized in the interest of their own profession the norms of generosity which social opinion demanded of the true knight, whereas clerical writers put much more stress than the laity upon obedience to religious rules and ideals.

to the particular values or systems upon which it bears, but the range and strength of its practical influence upon those values or systems. The former may be ascertained by finding out how wide the community of people is who, while dealing with the values or systems in question, accept this generalization; the latter by investigating in particular cases how far the behavior of these people is actually in agreement with the generalization, or if not, how the conflicting ways of behavior are dealt with. But the latter is only a question of degree; a generalization commonly accepted within a social community will always have some degree of practical influence. And the more uncritical, the less individualistic and independent of mind is the person from whom the sociologist takes his generalization, the better material this generalization is, for such a person is only the echo of the most potent and deeply ingrained views and prejudices of his milieu.

A different matter is the use of generalizations which are not intrinsic and directly influential parts of collective social life, but theoretic views of individuals who from a number of particular cases draw general conclusions about all the cases of the same kind. More caution is needed in utilizing such generalizations than in dealing with any other human statements, and too little caution is commonly used. For, in order to draw a valid generalization, one must not only observe objectively and thoroughly individual cases (which, as we have seen above, is not always easy), but apply the proper method of scientific induction in order to pass from data to general truths. Take at random a number of general statements found in the most serious newspaper and magazine articles, books of travel, reports of governmental committees, informatory works on

modern social and political problems, and see how many of them have been formulated in accordance with the logic of inductive research. Only a small minority of averagely educated people are capable of using the exact inductive method consistently, and out of these there are many—journalists, politicans, advertisers, propagandists, preachers—who actually do not care to use it, since for them the general views they express, just as the particular facts they quote, are merely weapons for social struggle. The sociologist must clearly realize that any general theoretic statement he uses as truth, whether his own or other people's, must be defensible on scientific grounds. He may admit statements on somebody else's authority, but it must be *scientific* authority; he need not verify a view he quotes when he knows that the person who has expressed this view normally complies with logical standards in formulating general statements, but otherwise he must. The still wide-spread practice of utilizing not merely facts, but generalizations of essayists who write about contemporary life, historians, travellers, etc. as foundations for sociological theory should be entirely stopped, as well as the still more pernicious modern custom (which is not justified by any lack of material) of quoting as authoritative statements theoretic opinions of prominent persons engaged in various domains of social activity.

The only justifiable way in which theoretic views of individuals who are not scientists can be used is to treat them as presumptions drawn by these people from previous experience for practical orientation in their social environment. If they actually apply these presumptions as technical rules in trying to influence their environment and are moderately successful, this is a

probable indication that their theoretic generalizations, however unmethodically reached and inexactly expressed, contain "a grain of truth," correspond to some objective uniformities of facts. Without taking them at their face value, the scientist may utilize them as provisional suppositions, helpful in formulating better defined problems and reaching more valid hypotheses.

With this question of utilizing other people's theoretic views, we have already reached our last problem of sociological method: how should the sociologist proceed in forming his own theoretic views on the basis of the material given to him from direct or indirect experience and observation at first or second hand?

References

There are a number of books and papers dealing in part or entirely with the general problem of sociological material and factual evidence, for example:

Durkheim, E., *Les règles de la méthode sociologique*, Alcan.

Richmond, M., *Social Diagnosis*, New York, 1917.

Chapin, F. S., *Field Work and Social Research*, New York, 1920.

Giddings, F., *The Scientific Study of Human Society*, Univ. of North Carolina Press, 1924.

Bogardus, E. S., *The New Social Research*, Los Angeles, 1926.

Lindeman, E. C., *Social Discovery*, New York, 1924.

Lundberg, G. A., *Social Research*, New York, 1929 (good bibliography).

Essays on Research in the Social Sciences, Brookings Institution, Washington, 1931.

Rice, S. A. and Brinton, H. P., "The Problem of Sociological Source Materials," *Publications, Am. Sociol. Soc.*, 1932.

Many valuable partial contributions to these questions are to be found in *Sociology and Social Research, Social Forces, American Journal of Sociology, Kölner Vierteljahrshefte für Soziologie, Zeitschrift für Völkerpsychologie und Soziologie* (since 1932, *Sociologus*), *Journal of Educational Sociology*, and other periodicals.

The problem of psychological evidence as "internal" and "immediate" evidence was raised already by St. Augustine, and modern philosophy since Descartes is full of discussions on the subject. In latter days, every new current in psychology has raised it in some form or other. For instance, Wundt did so when initiating experimental psychology; James introduced interesting points, particularly in connection with his *Psychology of Religious Experience;* Bergson gave the whole matter a special philosophic significance from his point of view in his *Essai sur les données immédiates de la conscience.* Münsterberg solved his conflict between naturalism and Fichtean idealism by contrasting scientific psychological evidence, reflective and secondary, with the fundamental evidence of the active and evaluating subject. The psychoanalysts emphasize the unreliability of conscious introspection as being a distortion of unconscious processes conditioned by social repression. The *Gestaltpsychologie*, with Koffka, Kohler, Wertheimer, opposes the primary and dynamic character of the experience of totalities to the secondary and static experience of the elements into which these totalities are resolved in experimental introspection. Finally, John Watson rejects not only introspection, but consciousness altogether. (See,

besides his main works, Watson and MacDougall, *The Battle of Behaviorism,* New York, Norton.)

On the phenomenological approach in general, see, e.g.,

Husserl, E., *Logische Untersuchungen,* Halle, 1900-01.
Scheler, M., *Versuche einer Soziologie des Wissens,* Munich, 1924.
——— *Schriften zur Soziologie und Weltanschauung,* Leipzig, 1922-24.
Litt, T., *Individuum und Gemeinschaft,* Leipzig, 1926.
Vierkandt, A., "Die Ueberwindung des positivismus in der deutschen Soziologie der Gegenwart" in *Jahrbuch für Soziologie,* II.

The fact that our observation in the cultural sciences in general is objective, though not sensory, because it deals with objective systems, was settled long ago by leading German thinkers and research students, beginning with Dilthey; and it seems really a pity that their very obvious results in this respect are simply ignored by many Americans. One may utilize these results without accepting the conclusions which some German thinkers draw from them as to the method of sociology. See, for instance, besides the works quoted above:

Weber, M., *Gesammelte Aufsätze zur Religionssoziologie,* Tübingen, 1922-23.
Dunkmann, K., *Lehrbuch der Soziologie und Sozialphilosophie,* Berlin, 1931.
Spann, O., *Gesellschaftslehre,* Leipzig, 1923.

Of course, even from this point of view the problem may be raised whether the observation of sociological phenomena has a claim to objectivity equal to that which a scientific investigation of knowledge, art, technics, lan-

guage, religion so obviously possess. For, if social phenomena have no cultural objectivity of their own, then the only alternative is between a subjective psychological interpretation by the scientist of subjective psychological processes, individual or collective (this is the standpoint of Wundt), or else sensual observation by naturalistic methods of material processes occurring in an agglomeration of organisms living in a natural environment, which substitutes instead of sociology demography, somatic anthropology, human geography and ecology.

There are various interesting attempts to justify the cultural objectivity of sociological phenomena: Simmel's formalism, v. Wiese's reduction to elementary, objectively definable inter-individual relationships, and the recent, essentially historical conception of social reality of H. Freyer in his important, but difficult book *Soziologie als Wirklichkeitswissenschaft*, Leipzig, 1930. I must confess that to me social reality is objectively observable, though not material, only because—like æsthetic, religious, technical, scientific, linguistic, economic reality—it is composed of specific, distinct systems whose elements are objectively given values and whose structure has an objective order of its own, though it is neither an order of ends and means, as in technical planning, nor an order of deductive construction, as in mathematics.

I trust, therefore, that the very progress of sociological investigation, by making this order increasingly evident to the serious-minded student, will contribute more to the disappearance of the sensationalism and materialistic metaphysics, now so frequent among American sociologists, than do philosophic discussions, which while strong on epistemology, are often weak when it comes to the methodology of positive scientific research. I greet, thus, as a hopeful sign, such recent methodological contributions as:—

Bain, R., "Behavioristic Technique in Sociological Research," *Publ. Am. Sociol. Soc.*, 1932.
"A Symposium on the Observability of Social Phenomena," *Sociologus*, 1932 (Cf. in particular M. J. Adler's paper).
Bernard, L. L., "Sociological Research and the Exceptional Man," *Publ. Am. Sociol. Soc.*, 1933.
Young, K., "Method, Generalization and Prediction in Social Psychology," *Ibidem.*

There are good works based primarily, if not exclusively, on one kind of sociological materials. These works well illustrate to what scientific use such materials can be put when properly handled. Books like Scheler's *Sympathiegefühle* or Vierkandt's study of social impulses in the first part of his *Gesellschaftslehre* show what good results can be obtained from personal experiences, original and vicarious, with a little addition of data from other sources. The works of Malinowski, W. I. Thomas' *The Unadjusted Girl* (Boston, 1923), J. M. Williams' *Our Rural Heritage* (New York, 1925), Thrasher's *The Gang* (Chicago, 1927), R. and H. Lynds' *Middletown* (New York, 1929), A. Blumenthal's *Small Town Stuff* (Chicago, 1932) illustrate direct personal observation with the addition of some vicarious personal experience and the experiences of other people, leading to explicit or implicit sociological generalization. A number of monographs that are based primarily on questionnaires might be quoted; but, though I wrote one myself, I believe that it is better to concentrate rather on comprehensive life-histories, as Clifford Shaw and a few others are doing in America, Chalasinski and Szuman in Poland. Naturally, observations by other people have been more extensively used in sociology than any other kind of material; sociologists have succumbed here

to the traditional prestige of historical erudition, and there are still some who believe that the supreme standard of scholarship is to know and to quote everything that has ever been written about the given matter, particularly when a lower society is concerned. There are, nevertheless, first-rate works based chiefly on second-hand sources, such as Durkheim's *Les formes élémentaires de la vie religieuse* and *Le suicide*, Briffault's *The Mothers*.

However, a methodologically perfect sociological study would utilize all varieties of sources: personal experiences and observations of the sociologist to formulate general hypotheses, experiences and observations of other people to determine the range of their applicability, selecting wherever possible such experiences and observations as can be experimentally tested by himself and others. But a methodologically perfect sociological study has yet to be written—and probably will not soon be written.

At this time, one of the greatest scholarly services that could be rendered to sociology would be a critical and selective survey of published materials and of sociographic studies based upon unpublished materials. Such a survey ought to be made from the point of view of the actual worth of the material or sociographic study for the purposes of sociology as a special, theoretic, analytic and generalizing science, not from that of a general theory of culture, nor from that of historical and anthropological knowledge of mankind, nor yet from that of social practice.

Nowadays, a sociologist who wants to find what good material there is on any problem belonging to his domain that is neither too general nor too special—say, neighborly relations, or the social rôle of the merchant, or the causal dependence of changes in group organization on changes in membership—must look in three kinds of sources: the

ethnology of lower societies, the history of higher societies, and the literature on contemporary life.

In ethnology, he does not find the introductory source books, encyclopedic works and existing bibliographies of much assistance beyond a preliminary orientation; he must turn to the great collections, periodicals and books of ethnography, folklore and travel. After wading through several hundred contributions, he discovers that ninety per cent of them are sociologically worthless, containing only very fragmentary or very unreliable information on social phenomena in general; that nine per cent, though dealing with some social matters, have no data bearing on his particular problem, and only one per cent can be safely utilized. And unless he becomes a professional ethnologist, he never knows what valuable material he may be missing among the thousands of contributions he has no time to survey.

Similar is the situation in history. The great majority of sources which interest the historian are irrelevant for the sociologist. Many of them have little, if any, bearing on sociological problems—as most of the monuments of material culture. Others are too brief, scanty and schematic to be of any value for sociological analysis, like most legal documents from the Middle Ages and antiquity. Others again may be numerous, comprehensive and reliable, but lack variety: thus, a small, but select part of all the existing descriptions of wars would be enough for the sociology of war. On the other hand, the very periods and problems on which there is the greatest amount of good sociological stuff often interest the historians but little, as there are few puzzles to be solved, and it is much more difficult to synthetize historically such rich data as those, say, on Europe in the nineteenth century than such scanty information as that on Europe in the ninth century. Guides

to historical sources and collections of sources are, therefore, as inadequate from the point of view of the sociologist as ethnographical guides and collections.

For it must be always kept in mind that the function of sociology is not to learn all that might be learned about the occurrences of a certain kind of phenomena (as, e.g., Westermarck tried to do about the forms of sexual relations) or all that might be known about any particular culture area, or any historical period or nation, but to investigate good instances of specific social systems and their changes, no matter when or where such systems occur. The selection of ethnological and historical material for sociological analysis must be guided in a considerable measure by the wealth and reliability of the material, whenever and wherever available. The sociologist who studies conjugal relations need not waste his time in collecting whatever scanty references may be found about them among a thousand lower peoples, when he can find exhaustive and reliable descriptions of a score or two of them; he need not bother to reconstruct the conjugal relations of ancient Assyria, or of Hellenistic Asia or of the Dark Ages in Germany, when he has adequate data from a dozen other periods and nations.

As to published materials concerning social systems which still exist in present civilized societies, there is certainly no scarcity of them; but there are several factors which make the great majority of them almost worthless to the sociologist. One of these is practical interests: publicity has become so important a method of exercising social influence that most information which is getting published is propaganda for or against certain purposes. For instance, in the course of an investigation during the academic years 1931-2 and 32-3 in connection with Columbia University and Teachers College, I could not find in American litera-

ture a single objective description and history of an industrial or political group. Even more detrimental for sociological purposes is the enormous predominance which economic data seems to have over social data in the minds of most modern observers. Thus, the majority of "social surveys" are so absorbed in economics and demography that they pay but little attention to such specifically social phenomena as the composition, structure and function of the numerous social groups existing in the given community, the rôles played in community life by different categories of social persons and the formation and differentiation of these categories, the mores and methods of social control, the multiple and various social relations between individuals.

Finally, even among those who approach their data in a spirit of perfect objectivity and are really concerned with specifically social phenomena, the majority, under the influence of the statistical bias, publish only materials collected in view of a statistical tabulation and consequently so schematized as to be of no value for sociological analysis. For example, with all the research that has recently been made in teachers colleges on the social life of children, it is difficult to find in any published books on the subject actual concrete information on particular children's groups such as would add something to what we know already from older descriptions.

It is, therefore, very important that sociological scientists, particularly those of the new generation, be not only spared the tremendous waste of time involved in looking through masses of worthless material, but be shown where they can find really valuable sources which they would otherwise inevitably miss. And, furthermore, such a survey will show what necessary materials are still lacking, and thus direct observation in the future.

CHAPTER V

Criticism of Some Methodological Tendencies

1. *Preparation of Material for Scientific Use*

Sociological material, like any other scientific material, comes from its empirical sources in a chaotic condition and must be prepared for scientific use. This preparation involves several stages.

The task of sociology is to study social systems, not other kinds of cultural systems. Now, in concrete cultural life, such as we find it mirrored in our sources, social systems usually appear connected and intertwined with various other systems—economic, technical, linguistic or religious. Social actions are performed for economic, technical or religious purposes, as when people try to force or coax other people into serving their ends of greed, efficiency or sanctity; economic, technical, religious actions play an auxiliary part with regard to social actions, as when a social worker gathers funds to pursue his ends with, a war department promotes technical invention for wholesale killing, a Henry IV embraces Catholicism to become king of France. Social relations are influenced by non-social factors; take the influence exercised upon the conjugal relation by the economic condition of the parties. And *vice versa*, many non-social pursuits in which steady human cooperation is necessary require regulated social relations

in order to be achieved and are influenced by every change in them: technical production in capitalistic countries is essentially dependent on the relationship between employer and employee—a specific variation of the older relationship of master and servant; in communist Russia it depends on the relationship between the group and group member. In "professional" positions, like those of the scientist or artist, the social interaction and determination of the human person is closely interconnected with scientific or artistic interests and pursuits. There are groups which exist almost exclusively for the common achievement of economic or technical ends, like a stock-company or a factory group, while on the other hand almost every group needs economic and for the most part also technical means for its corporate existence.

Thus, the first step of sociological research is to segregate its raw material, to pick out the specifically social data, leaving the rest for eventual consideration later on.

Secondly, even social systems in concrete experience and observation are variously combined into complexes. Every human individual belongs to several groups, occupies several independent positions, is party to a number of social relations, performs many independent social actions; every human collectivity, e.g. the population of a city, forms many groups (some connected, some unconnected), has numerous and varied positions for individuals to occupy; there are innumerable social relations existing in its midst, and infinite social actions are performed. Any fragment of social reality coming under our observation is apt to contain a number of partly interdependent, partly merely coexisting social systems, and no efficient scientific study can be made of

any of them, unless their complexity is unravelled by a preliminary analysis, particular systems are isolated in abstraction, and their connection apprehended.

Thirdly, we are seldom capable of finding all the data necessary for an exhaustive knowledge of any particular social system. While our raw materials usually contain much we have no use for, there are also usually some data lacking that we cannot dispense with. Such lacunae must be filled in some way or other. It is well if we can obtain the necessary data by going back to our source and making additional observations, experiences or inquiries. More often, however, this is impossible. In such a case there is nothing to do but to fill the lacuna by means of *conjecture*. This is not necessarily as risky as it sounds, but depends on what other evidence we possess. Conjecture is, of course, entirely different from *hypothesis:* the latter is a tentative theory concerning characters common to many objects or the causes explaining many facts, whereas the former is simply a tentative assumption of the existence of some particular object or the occurrence of some particular fact. Every conjecture is based on analogy. If we find a social system otherwise similar to some system we already know, but with some element seemingly missing which the latter possesses, we conjecture that this element is there too, but has escaped observation or not been reported in description, for we presume that otherwise the whole system would be different. The conjecture is, of course, more or less reliable, depending on the degree of similarity between the two systems and the relative importance of the missing element for the structure of the system. In fact we are continually, though mostly unreflectively, using conjecture with

good results not only in theory, but in practice, and we could hardly do without it.

Separation of the kind of system to be studied from other systems; analytic determination of elementary systems involved in concrete complexes; conjectural filling of lacunae in the materials concerning particular systems: these three functions belong to the preparatory stage of scientific work, called in French intellectual slang the "*cuisine scientifique.*" In mature scientific publications, this is left outside and only the prepared materials presented, unless there are some special difficulties about their limitation, analysis, or conjectural interpretation. In sociology, however, there has been so much arbitrariness in handling material that (as a natural and in some measure justified reaction) sociologists, particularly in America, began to present their materials in full detail so as better to subject their conclusions to the control of the readers.

However desirable such a procedure may seem at first glance, it is really most uneconomical and at the same time superficial. Of course, this does not apply to books meant to be mere collections of materials for general use in which any comments made by the editors do not pretend to be theories utilizing these materials fully, but mere indications or examples of their possible utilization. But an author who, to solve one problem, quotes at length concrete material which could be used to throw light on a dozen problems, wastes material and printing space. He saves, indeed, his own time and energy, for it is much easier to publish raw materials than to prepare and utilize them carefully; but such a saving is hardly desirable from the point of view of scientific progress. Incidentally, such a method favors the development of superficiality in scientific work.

Since most readers like concrete material—some because it is easy and interesting reading, others because they can turn it to various scientific uses of their own—an author who gives much new material is sure of a good reception, even if his own theory should be most vague and superficial, and fail to do justice to the material.

It is obvious that already in collecting material we should keep in mind the scientific purpose for which it is to be used, and even more so in preparing it for use. While it is not impossible to sift, analyze, and supplement data concerning particular social systems with no other purpose than to describe these systems as thoroughly as can be done, still, as we have seen before, this kind of descriptive interest cannot be self-sufficient, for every particular description presupposes former generalizations and is a contribution to future generalizations. Particular scientific material is meant to serve for the construction of abstract and general scientific theories, while abstract and general scientific theories are meant to serve for the rational interpretation of particular real data. Though the way of using data for theories has often been described—and prescribed—in books of logic, it might be well to mention one or two important landmarks which are apt to be ignored in sociological research and to which even logicians do not always pay the attention they deserve.

2. *Sociology as an Independent Inductive Science*

The starting-point of all sociological research must be the firm and clear realization that sociology is an independent empirical science. This means that the only ultimate foundation of sociological theory is *empirical social data.* No sociological theory can be based on con-

clusions drawn from non-sociological theories, nor can any but social data serve to establish sociological truths. Many misunderstandings would have been avoided, much waste of time and energy prevented, if this point had been originally and consistently kept in mind. It permits us to reject at the outset all sociological conceptions and arguments founded not upon *empirical data studied* by sociologists, but upon general and abstract *scientific truths* reached and recognized in some other discipline—psychology, biology, anthropology or geography. It makes us exclude from our field all theories which, while claiming to be sociological, have been built for the most part not upon the study of *social data,* but upon that of some *other kind* of cultural data—as, for instance, the theories which interpret social life in economic terms of satisfaction of needs, in terms of material technique, magical, religious and intellectual beliefs, or a combination of these.

The second point which must be firmly established at the outset is that sociology can be nothing but a strictly *inductive* science. This does not mean that it should not use deduction: no science can live without the help of deductive reasoning. Nor do we wish to exclude from its field the method of phenomenological analysis which has been recently distinguished from both induction and deduction. We simply wish to emphasize that induction is the dominant and determining method of sociology: deduction must remain entirely subservient to it as an auxiliary method, while phenomenological analysis is only one of the stages of induction.

This is, of course, not the time nor the place to weigh the logical and epistemological claims of the deductive and inductive methods. The fundamental distinction between them is that from the point of view

of the deductive method the final test of a new truth is its logical agreement with a truth already established, whereas from the inductive point of view the final test of a new truth is its validity in theoretic application to empirical facts. It is obvious that a science dealing with empirical reality could not be deductive, for it would either be incapable of proving anything or else be inapplicable to its object-matter. Indeed, a deductive science needs a set of fundamental truths—axioms—which would serve to test all the others, but would not need to be tested themselves. These axioms would be derived either from experience or from some other source. If they were derived from experience, they would be inductive, and therefore from the point of view of the deductive standard should be again tested by deduction from other truths. If not, they would be inapplicable to empirical reality, unless that reality were first made to fit them, which means that it would not be empirical reality, independent of mind, but the work of mind. The latter is the case with the applicability of mathematics, not because (as Kant assumed) mathematics deals with pure space and time, which are conditions *a priori* of all experiences, but because to make reality fit into mathematical formulae we substitute symbolic schemes for real things and processes.

The phenomenological school mentioned in the previous chapter rejects the alternative here stated by claiming that axioms have absolute certainty, not to be obtained by induction and not derived from other truths, and yet are applicable to experience. They are intuitively apprehended as self-evident and need no other proof, nor can any be given; on the contrary, their validity as guaranteed by their evidence is the condition of all demonstration, deductive or inductive.

Far be it from us to deny the primary and fundamental importance of intuitive evidence; we agree that it underlies and conditions all demonstration, but for this very reason it is not a special method. Intuitive evidence is the mark of applicability of abstract truth to concrete fact, and it is nothing else. There is no intuitive evidence in the relationship of abstract truths as such: to make abstract logical reasoning intuitively evident, we must illustrate it with concrete examples.[1] Nor is there intuitive evidence in experience as such: evidence is a test of validity, and experience is neither valid nor invalid—it is simply there. What is intuitively evident is that a judgment does or does not apply to an empirical datum; intuitive evidence alone permits us to test truths by their theoretic application to facts. And the term "theoretic application" means nothing else and nothing more than that a judgment is true or false with respect to a certain empirical fact. The whole problem why and how knowledge in general is applicable to reality, can accord with reality, is a pseudo-problem, which becomes an actual problem only when reduced to specific methodological questions. The difference between the phenomenological and the inductive method consists in a different appreciation of the scientific value of intuitive evidence. The inductive scientist, while trusting intuitive evidence to prove that a theory is applicable to any empirical case in particular, does not trust it alone to demonstrate that the theory is true in general.

Scientific induction in its best form may be said to

[1] Examples may become unnecessary after we are sufficiently trained in using symbols, as in mathematics or symbolic logic; we will then substitute for logical evidence simple experience of symbolic relationships and systems of symbols.

combine deduction and intuition into a higher dynamic unity. But it can do this only if it substitutes throughout scientific research hypotheses, or relative truths, for absolute truth. And since such a substitution goes against the grain of the spontaneous dogmatism inherent in practical common sense and in formally deductive ratiocination, it has been achieved with much difficulty and hesitation and only in the most advanced sciences—physics, chemistry, and certain parts of biology. In sociology inductive research is barely beginning to reach this stage; most of it still goes on within the old logical frame work of popular mediaeval scholasticism only superficially modernized. Most sociologists apply only the so-called *enumerative induction*, either in its primitive type of common-sense generalization or in the more precise form of "statistical method."

3. *Enumerative Induction*

The method which we call enumerative induction (the scholastic *inductio per enumerationem simplicem*) has grown out of the primary intellectual need of simple, secure and stable mental rules for practical orientation. Its earliest results in the field of social knowledge are popular proverbs, most of which express generalizations concerning social life, connected or not with practical advice: "honesty is the best policy," "birds of a feather flock together." It has continued to be used, with very little change, in ethical and political works from antiquity down to present times, whenever an author, not satisfied with deducing rules of conduct from principles accepted *a priori*, attempts to base his views on experience and observation. Historians have used it, too, in most of their generalizations, though

among them we sometimes find rudiments of a different and more fruitful method.

When sociology began to emerge as a strictly theoretic discipline in opposition to ethics and politics, and as a nomothetic discipline in opposition to history, enumerative induction sprang into unprecedented prominence; it remained for a while the only sociological method, after philosophic deduction and historical description had been (in principle, at least) eliminated. Spencer's *Principles of Sociology*, Westermarck's *Origin and Development of the Moral Ideals*, lately Sumner and Keller's *Science of Society* will in all probability always remain classical examples of this method at its pre-statistical level. Before passing to the modification of enumerative induction which statistics has introduced, it will be well to characterize its general presuppositions.

It is dominated by that conception of knowledge which regards truth as the final and unshakable result of research past and done with, to be applied as a practical guide or contemplated with Platonic enjoyment, as one wishes. Induction from this point of view is an attempt to discover some final truths about a certain class of empirical data, circumscribed in advance, by studying a number of cases belonging to this class. Originally and fundamentally, the truths sought for are to be characters common to all the data of the given class and only to these. Such a problem, however, if real, is insoluble; and, if soluble, is fictitious.

Indeed, if you really do not know in circumscribing a class what are the common and distinctive characters of all the data of this class, you may be sure there are no such characters; and consequently, that your class is scientifically worthless. Whereas, if you circumscribe the class by defining it, if you already know that all

the data of this class are in certain respects essentially similar among themselves and essentially different from all the data of other classes, whatever common and distinctive characters you may find will be already implied in the exclusive similarity which has served as a basis of your definition; this means that your discoveries will be purely illusory, will consist at best in making explicit what was already implicit in the definition.

This is not a mere formal quibble which might be waived in concrete scientific research, but a fundamental deficiency of method which is materially demonstrated at every step in traditional sociological studies. There is not a single sociological generalization applicable to all the data of a class and only to the data of a class defined in advance that is not implied in this very definition. You cannot expect that reality will fit into any logical scheme you may apply to it when you circumscribe for investigation a number of data in defining them "formally" by some superficial marks, unless your definition is in fact "material," not "formal," and the marks have been shown by previous research not to be superficial, but to result from the fundamental nature of these data.

Suppose, for instance, you want to study the class called "criminals" or "offenders." If you define an "offender" formally as a person who has committed at some time an act forbidden under penalty by the rules of some group, or more narrowly by the law of some state, you may be sure that not a single feature will be found common to all those persons and no others except that very formal feature of their having performed that kind of act. Whereas, if you define the "criminal" as a "born criminal," as a certain anthro-

pological type in the sense of Lombroso, and include in your study only those who represent this type, you will find a number of features or combinations of features common to and distinctive of "criminals" in this sense, but these will be features implied in and resulting from Lombroso's anthropological structure. In a word, you can find nothing in the definition of a class that you have not already put into it.

This is why sociology, since it has been trying to be inductive, has usually been satisfied with looking in any given class for *relatively prevalent* characters rather than for common and distinctive characters, and has formulated "approximate" generalizations of the same type as popular common-sense reflection, that is, generalizations applying to "many" or "most," but not to all the objects or processes of a certain class as distinct from other classes. Every generalization found in older sociology that is not deduced from premises arbitrarily created or borrowed from some other discipline explicitly or implicitly admits exceptions. And these exceptions are not merely apparent, explicable by a combination of the given general principle or law with some other general principle or law, but real, inexplicable, because resulting in each case from a unique combination of particular concrete circumstances. There seems to be nothing to do about them, but just to accept them as unpleasant proofs of the irrationality of the empirical social world, and say as little about them as possible.

The progress from common-sense enumerative induction to scientific enumerative induction consists simply in an increase of thoroughness and circumspection. Where a common person generalizes from half a dozen instances, a Westermarck will quote threescore.

And while a high school student in discussion admits exceptions to his generalization only when forced to it by his opponent, a mature writer mentions their possibility in advance or implies them in the very form of his statement. The sociologist's judgments about human affairs are thus more reliable than the untrained thinker's, both because his enumeration embraces a large proportion and variety of data belonging under the given class, and because his conclusion does not pretend to apply to all the data of this class and thus cannot be contradicted by merely enumerating some exceptions.

Still, there is an obvious limitation to his reasoning. Unless he has actually investigated the majority of the data of the given class S, he cannot even affirm rightfully that most S are P; but only that "some S are P." If he concludes from the latter to the former his judgment remains unreliable because somebody else may investigate an equal or larger number of data and come forward with the claim that "most S are non-P," or "most S are Q," Q being a positive character exclusive of P. This is what we usually find in older sociology when the number of data belonging under class S is large; and it still often happens. Here is where the statistician comes in with his ambition to substitute "objective" and "exact" for "inexact," "subjective" and "vague" judgments.

4. The Statistical Method

The statistician begins with the obvious assumption that in order to be sure that "most S are P" you must *count* the S that are P, and compare their number either with all the S or with the S that are non-P. Observers

may argue indefinitely, for instance, whether most families at a certain period in a certain country derive their livelihood from agriculture or from other pursuits, until the statistician comes and decides the matter finally by counting. Furthermore, for such vague judgments as "the greater part," "most," "almost all," the statistician can substitute definite proportions, and say that 60%, 80% or 95% S are P; this, again, permits him to calculate exactly the degree of probability for any S's being P. Though we are here as far as ever from the general judgment "all S are P," which would permit us to assume with absolute certainty that every S is P, at least we have reliable knowledge how far we are from this limit. Of course, a judgment "80% S are P" does not yet characterize the class S as distinct from other classes; whereas in starting to investigate a class circumscribed in advance by a "formal" definition, such a characteristic is aimed at. In popular generalizations it is usually implied that the characters mentioned as common to a class are also distinctive; if they were not, they would not be worth mentioning. If anything is said about "clergymen," "politicians," "families" or "towns," it is meant to refer specifically to these, not to other possible objects of reflection. Many sociological generalizations are also only implicitly exclusive; as, for instance, all the statements referring to "primitive man" or "primitive societies" as implicitly differentiating these from among "men" or "societies" in general. Here also the statistical method has brought an increase in reliability and precision by making such limitations explicit and testing them. The class S is explicitly regarded as belonging under a wider class O, and the judgment "80% S are P" is considered fully significant only if it can be shown that in the whole class O a

certain number, say 40%, are P; or that among the O that are not S, 75% are not P.

This increase of reliability and precision concerns also those generalizations in which, complete enumeration being impossible, instead of the indefinite totality of data a certain definite number is substituted as representative of this totality. We obviously cannot count all the offenders or even all the convicted offenders, past, present and future, in the civilized world; but we may select several thousands of them in accordance with definite rules of method and try to find whether there are any personal characters which can be statistically shown to prevail among them, while not prevalent among an equal number of otherwise similar non-offending or non-convicted persons. If such characters are found and there is a marked difference in their distribution among the selected offenders as compared with non-offenders, the conclusion may be drawn that approximately the same difference would be found if the whole class of offenders and the whole class of non-offenders could be counted and compared. Whatever doubts may still linger about the reliability of this conclusion, the latter is (providing the proper rules of statistical method were observed in forming it) surely much more reliable and obviously incomparably more precise than the judgment, say of a judge who, having convicted a number of felons in court and been in contact with a number of non-convicts in his social milieu, concludes that most felons are stupid and greedy, confidently implying that most non-felons are neither.

And yet, notwithstanding all the progress that has been made in enumerative sociological induction from a Greek sophist to a Voltaire, from a Voltaire to a Quètelet, from a Quètelet to an Ogburn or a Dorothy

Thomas, its general methodological character remains unchanged. This progress has been achieved primarily by improving the *formal certainty* of inductive judgments; secondarily, by increasing their *formal precision.* The question of their *material significance* for the advancement of knowledge was left out of consideration, and in the course of time this significance has come to be cheerfully sacrificed. With one notable exception, which we shall characterize and explain presently, all the applications of social statistics for a hundred years have done nothing but *formally* prove or disprove already existing common-sense judgments of more or less shrewd politicians, business-men, novelists, moralists, public-house or drawing-room philosophers.

Yet it is scarcely surprising that the main current of sociological development has gone along this line: it was the line of least resistance. The intellectual habits of the sociologist himself, who from childhood on has been used to hearing and expressing only such generalizations about social life as were based on enumerative induction, have formed a powerful obstacle against deviating from the old and well-beaten path; the obvious thing to do was simply to make the path safer. And here the scholastic respect for the apparent security of Aristotelian deduction, lingering until quite recent times, has combined with the growing admiration for the apparent perfections of mathematics as a scientific tool. The present domination of statistics appears thus as a perfectly explicable result of the cooperation of factors which it would have been difficult to withstand until their bearing became fully manifest.

And this bearing is in deep opposition to what may be regarded as the leading tendencies of modern science in contrast to both scholasticism and Cartesianism. These

tendencies, manifested most clearly in experimental physics, chemistry and biology, can be characterized as a combination of a searching interest in empirical data with a dynamic ideal of rational science. Scientific interest in empirical data involves the belief that any fragment of concrete reality is a potentially inexhaustible source of new discoveries—discoveries intensive, rather than extensive—new elements and aspects of familiar data rather than totally unfamiliar data. The true inductive scientist never is satisfied nowadays with what he already knows about an empirical datum, never thinks that his definition of it is conclusive and expresses its ultimate essence; he is always watching for an opportunity to analyze it more thoroughly and from a new point of view, to dive deeper and in some new direction into the complexity hidden under its apparently simple surface and thus to bring to light some unexpected treasure. The dynamic ideal of knowledge is correlative with this attitude toward reality. The modern scientist appreciates the quest for new knowledge more than ready and recognized knowledge, a hypothesis that leads to new problems more than a certainty unshakably connected with other certainties. A truth is to him intellectual activity helping to create new truth; it is not a timeless object of mental contemplation nor yet a passive instrument for practical applications.

Now, enumerative induction culminating in the present statistical method is opposed to both these tendencies. The task of determining what characters are common to the majority of data in a class which has been defined in advance conflicts with that searching analysis of each particular datum to which the progress of modern science is due. The single characters that are

to be statistically tabulated must be simple and easily discernible on superficial observation; otherwise, if a thorough study of each case were needed to ascertain their existence or non-existence, it would be practically impossible to fulfil the task of statistics except for a class containing only a few data and consequently not in need of statistical investigation. Each one of such characters is apt to be of but little value for the knowledge of any particular fact, unless we have already shown by previous intensive studies of such facts that it is a part or a symptom of a combination of essential characters distinctive of these facts as a class—and then all statistical tabulations are superfluous, since we know already more than they can teach us. Take such a character as "ground for divorce" in divorce statistics. The statistician for obvious practical reasons must rely on the "grounds" alleged in the plea and accepted in the court's decision. Needless to point out, in many cases this character throws no light whatever on the process which has culminated in divorce proceedings; in many others it is actually misleading. If we could show that, in a certain kind of divorce pleadings, a definite ground is symptomatic of the real facts which produced the divorce, this would mean that we know about this kind of divorce incomparably more than any statistician could ever discover.

Even if it were possible to find some criteria by which the scientist could determine in advance which of the characters accessible to statistical study are more and which are less important, there would still remain a fundamental limitation in the statistical method which can never be overcome: it is the practical impossibility of taking into account combinations of more than a few characters—combinations of four characters are already

very difficult to handle statistically. This limitation prohibits absolutely any progress in the intensive study of facts.

The statistician has, indeed, the obvious retort ready that he can handle any number of characters in separate tables. He may collect information on as many as twenty characters and then deal with combinations of any two, three or four at will. But this is beside the point. For the characters (aspects or facts) of any particular system, object, process, are not detached entities: they belong together and are mutually interdependent; knowledge of the system, object, or process does not mean knowledge of each of the characters separately, but of all of them together as interdependent. Any progress in knowledge involves not only the discovery of new characters, but also a different and better understanding of the way all the characters, new and old, are combined in the given system, object or process.

The statistical method substitutes for this real, objective interdependence of all the characters of an empirical datum a multiplicity of arbitary mental combinations of characters artificially isolated from their empirical context. By making the study of facts subservient in advance to its final purpose of a mathematical play with symbols, not only does it fail to stimulate progress in the analysis of these facts, but actually obstructs it.

Thus, the worst mistake of mediaeval scholasticism is here repeated: juggling with concepts instead of investigating reality has to be again accepted as the essence of science. That the rules of the intellectual game are now those of mathematics rather than of Aristotelian logic is a minor difference. Nor can statistics claim any superiority over scholastics on the ground

that its concepts are based on real data, for so were those mediaeval concepts which concerned the empirical world. Outside of the lunatic asylum there are no theories unsupported by facts. The difference in this respect between scholastical and statistical methods on the one hand and those of modern inductive science on the other hand consists in the fact that the former tend to a minimum, the latter to a maximum of thoroughness in the study of facts.

The opposition between the statistical method and the modern ideal of knowledge manifests itself further in a systematic avoidance by this method of any conclusions which might be challenged and thus lead to the formulation of new problems. In this respect it plays the same rôle in science as eclecticism in philosophy and religion, or opportunism in politics: it stems the current of new ideas by making every idea unproductive. And this result is due to the way the statistical method handles exceptions. The only reason for the existence of this method is, as we have seen, the impossibility of arriving by enumerative induction at judgments of the type "all S are P," or rather, "If *p*, then *q*." Whenever such judgments can be formulated, the use of the statistical method is precluded. Now, everybody knows that in physical and biological sciences there are innumerable judgments bearing upon empirical reality which have this form. Every botanist or zoologist in describing a species means to characterize all the living beings of this species; every physicist or chemist in formulating a law claims that the law is applicable to all the processes of a certain kind Does this mean that the reality with which the botanist or the physicist is dealing is so uniform empirically that no exceptions, no deviations from the type can ever be observed? Of

course not: the botanist or the physicist well knows that cases may be discovered which will contradict his generalization. But he is not afraid of them. He is ready to grant any exception as raising a problem and thus stimulating new research. The research may enlarge and confirm his theory by helping to discover a new species or a new law definitely connected with his previous generalization—which will mean that the exception was only apparent. Or further research may invalidate his former generalization and force him to reach wider and deeper in creating a new and more efficient theory.

The exception is thus an essential instrument of scientific progress. But it is this only because it is not meekly accepted in advance as a necessary limitation, imposed by facts upon the logical perfection of the theory, and which forces the scientist to substitute approximate instead of exact judgments. Truly creative science does not admit that any empirical obstacles can prevent it from performing its proper function; and this function consists in rationalizing reality, constructing perfectly coherent systems of abstract and general concepts by which concrete and particular data are classified and explained. Science is reason challenging experience and forcing it into a rational order. An exception is a revolt of experience against reason. Statistical science, faced with such a revolt, passively relinquishes its claims and withdraws from the struggle into the realm of pure mathematical concepts, there to weave into arbitrary patterns (without any further interference from the real world) whatever shreds of theory experience has left to it. Creative science—physics, chemistry, biology —to subdue this revolt invents new weapons, and either supplements or supplants the theory that has met with the exception by a new theory as radical, or more

radical, in its claims, and thus turns defeat into victory, strengthening and widening the sway of reason.

The statistical method might be simply dismissed as a scientifically useless, but inoffensive amusement of the type of chess or crossword puzzles, if it were not for the social harm it is actually inflicting. Firmly entrenched in institutions of higher education and even more so in institutions of research, it exercises a highly undesirable influence upon the younger generation of students in sociology and neighboring fields. This influence consists in substituting tabulating technique for intellectual methods, and thus eliminating theoretic thinking from the process of scientific research. Not only is collecting materials for statistics a mere routine process—the more so the more perfect the technical rules become; but the whole intellectual activity of analyzing materials and drawing theoretic conclusions, which has always constituted and always will constitute the very essence of scientific research, is being gradually reduced to a minimum, as greater technical perfection is obtained. A condition can be foreseen—indeed, it has almost been reached—when anybody who has learned by heart the various technical rules and formulae of statistics, with no other scientific education whatsoever and no more intelligence than a moron, will be able to draw from a given material all the conclusions which the statistical problematization makes possible.

Some sociologists of the statistical school are aware of this trend in the evolution of their method and even explicitly acknowledge it. The rôle of creative thinking in science, according to their conception, will be reduced to the function of formulating hypotheses which are to be tested by technical means. But we have seen that the only hypotheses statisticians ever have formulated, and

ever can formulate, in view of the unavoidable limitations of their method, are no more than superficial generalizations of common-sense practical reflection. There is little place for creative thought and even less for scientific progress in this kind of problematization.

While the leaders of this movement away from intelligence are, of course, still using much intelligence to invent the technical devices to eliminate thinking, many of their younger followers are already taking advantage of the chance to pose as scientists with less mental effort than it takes to be shopkeepers or farmers.[1]

It ought to be clear that all this criticism is directed against the statistical method as a *method of inductive generalization* and has *no bearing at all* upon the use of statistics as an auxiliary *technique for measuring* such particular data as are measurable in terms of statistical units. We shall take up the question of the legitimate use of statistics in speaking of the quantification of social reality (Chap. VI. sec. 6).

5. *The Origins and Difficulties of Analytic Induction*

Enumerative induction, as we have seen, originates in the common tendency to reach quickly secure, even though superficial and inexact generalizations for the

[1] Two interesting by-products of this process already begin to be noticeable in colleges and universities. First, those students who have creative intellectual interests, but little chance to use their minds while dealing with facts statistically under the supervision of their research directors, indulge in abstract speculation uncritically and chaotically outside of their appointed work, thus learning bad thinking instead of good thinking during their higher studies. Secondly, wherever the statistical method definitely gains the ascendency, the number of students of a high intellectual level who are attracted to sociology tends to fall off considerably.

purpose of ordinary practical orientation. The same practical tendency obviously underlies the modern statistical form of enumerative induction: nearly all statistical "research" has political, economic or philanthropic aims in view. This is probably the main reason why enumerative induction has remained prevalent in the social and economic fields, where practical interest has always been paramount. "Practical" people are continually forgetting the lesson that quick results are seldom satisfactory, and that the purposes of practical control of cultural reality would be served best by a science entirely independent of these purposes,[1] a science which followed exclusively the two leads of a deep intellectual curiosity about particular data and an insatiable philosophic tendency to use acquired knowledge for the acquisition of new knowledge.

This is, indeed, how the other logical method developed which we call *analytic induction*, its development emphasizing what we believe to be its most important characteristic. We find it applied by Plato when he analyzed individual instances of figures drawn upon sand and concluded from this analysis as to the general properties of figures of the same type—in this way laying (or perhaps only perfecting) the foundations of inductive geometry. We see Aristotle absorbed in a detailed analytic study of individual specimens of animals, and utilizing every discovery to build the first systematic zoology. We find Theophrastus, too, who had, as he says, at the age of one hundred and five learned at last how to enjoy life, settling down to observe, analyze and compare individual men, thus

[1] For criticism of the practical applicability of statistical results, see the author's paper, "An Analysis of Social Processes," in *Publications of the Am. Sociol. Society*, 1932.

making the first really positive differential psychology. There is Galileo, who, after investigating thoroughly a few systematically differentiated cases of movements, drew conclusions which bear on all movements of a certain general type. And there are all those innumerable laboratory workers—some obscure, some renowned—who have made physics, chemistry and general biology, not by agglomerating large masses of superficial observations, but by inducing laws from a deep analysis of experimentally isolated instances.

Into sociology this method did not penetrate until recently. Le Play began by using it, but (being interested in practical aims) combined it with enumerative induction, and thus lost most of its advantages. The school which centered around Durkheim tried to use it consciously and planfully, but made the mistake of believing that a self-sufficient theory can be built on one instance thoroughly analyzed. This is what Durkheim did when he defined the essence of religion on the basis of a study of Australian totemism, and Czarnowski when he drew from the study of the legend of St. Patrick conclusions about the cult of heroes in general.[1] The same mistake has been committed by the phenomenologists harking back to the Platonic Idea under the direct influence of Husserl's logical reasoning.

In fact, William I. Thomas was probably the first who based sociological research entirely on the analysis of particular cases, utilizing several *different* instances for every generalization. He developed this method chiefly in his lectures from about 1905 to 1915 and so influenced many young sociologists. In his *Source Book in Social Origins* the method was already partly applied.

[1] Cf. E. Durkheim, *Elementary Forms of Religious Life*, and S. Czarnowski, *Le culte des héros.*

We used it together on a large scale in the *Polish Peasant*. The disciples of Thomas have since spread it pretty widely.

Thomas chose this method instead of the prevalent one of enumerative induction not because of abstract methodological considerations nor yet under the influence of older sciences. He was simply led by his own vivid intellectual curiosity and interest in particular cases, coupled with an incomparable genius for the selection and interpretation of significant concrete data. New methods are always initiated in this way—and not only in the field of science. But in order to develop fully all the implications of the new method and to raise continually the level of its exactness and reliability, its further use must be accompanied for a long time by critical methodological reflection.

As a matter of fact, little methodological thought has been expended on this new method in sociology, while the adherents of enumerative induction have never ceased to discuss the statistical method, to extol its alleged merit, and to improve it within the narrow limits of its possibilities. It is high time to correct this deficiency. But before trying to outline the leading ideas of analytic induction, it will be well to mention with a warning certain intellectual traditions which in all sciences did for a time, and in sociology still do, obstruct its progress and favor the domination of the older and less efficient methodological tendencies. These traditions are: the common use of words in an indicative rather than in a descriptive sense, and the current pseudo-deductive ways of exposition and demonstration.

In common speech, a word symbolizes logical extension rather than comprehension. In general, when people use words like "criminals," "marriages," "un-

employment," they are more interested in determining whether particular men are or are not criminals, whether a given couple are or are not married, how numerous the cases of unemployment are as against employment in a city or a county, than in learning exactly what a criminal, a marriage, or a case of employment or unemployment *is*. Even when they are conscious that they do not know the meaning of some unfamiliar word, they prefer to have several objects or processes indicated to which the word applies rather than to have an analytic definition of the concept given to them. This is because the primary use of words is for social communication, and it is easier to establish a community of objects indicated by the word than a community of its conceptual meaning.

Thence the common assumption that when any word *A* is used, the class A to which it applies has been already circumscribed, i.e., that any datum is already either A or non-A. Thence also the demand that when word *A* is used in discussion, it be defined in advance in such a way that everybody who participates in the discussion should include the same data under class A. But it is obvious that from a scientific point of view logical extension depends entirely on logical comprehension. Any object belongs to the Class A only if it possesses all those fundamental characters which all other objects belonging to class A possess, and which are comprehended in the concept A. The assumption that a certain word is applicable to all the objects or processes of a class, and only to the objects or processes of this class, is justified if we know already all the common and distinctive characters of this class and are using this word entirely and exclusively to symbolize this knowledge of ours. That happens only when the

word is a scientific term carefully selected and defined *after* a full and thorough study of the kind of objects or processes to which we wish to apply it. Until then the use of words as indicative of classes should be purely tentative and provisional.

The traditional and common demand that terms be defined in advance and consistently used in accordance with their first definition has not originated in scientific considerations, but in the purely social needs of discussion. Verbal consistency is necessary to avoid misunderstandings and waste of time in verbal disputations; consequently, it was sublimated at the time when intellectual life centered in small groups, like ancient and mediaeval "schools," and most of it expressed itself in personal intercourse. It remains, of course, a valuable rule for the communication of knowledge already achieved: but it is decidedly harmful if applied to knowledge in the making. It hinders the progress of inductive knowledge, particularly in those fields where, as in sociology, numerous popular terms became more or less fixed by long usage prior to any scientific research. The sociologist who uses any one of these terms (and no sociologist can avoid them) must be always ready to qualify it, to exclude from the sphere of its application data which he began by including in it, or to extend its application to data which at first he did not think of taking into account, or even to reject it—all depending on the results of his analytic studies. And in any case he must be sure that his final use of the term be very different from its popular use; if it is not, there is a strong presumption that his research has been as superficial as common-sense reflection.

In the course of research, the way of preserving a proper plasticity of the popular terms used is not to

define them at all, but to rely on the context for any shades of meaning one wishes to convey. If sociologists kept to this rule, and made only such definitions as follow logically from their theoretic investigations, much of the present terminological chaos would be avoided; for then every difference in terms would be significant of a theoretic conflict, and would thus stimulate further research to remove it. To bring this condition about, it would be desirable if sociologists even now learned to view every terminological definition as an implicit or explicit hypothesis, and instead of granting it, demanded that it be tested. Indeed, it should not be very difficult to correct the custom of using terms dogmatically—physical and biological sciences have done it with complete success.

More important is the other impediment in the way of analytic inductive science which, though it cannot prevent its progress altogether, hinders it considerably. We mean the traditional method of exposition and demonstration which modern sciences inherit from the time when perfect science was thought to be deductive, and efforts were made to give all valid knowledge a deductive form.

It is a fact that a sociologist, just as a physicist or a biologist, as soon as he passes from actual investigation to a systematic presentation of a definite body of knowledge concerning a certain object-matter, begins to proceed in an entirely different way than before. He formulates first his most general principles as if these were unconditional, basic truths on which the validity of all that follows reposed; then he orders his theses in logical sequence, as if each subsequent one were deductively derived from the preceding ones, and its validity were due to this derivation; and he quotes

particular facts as if they were mere instances illustrating the general truths. He has been taught in school and trained by reading older scientists' works thus to organize his ideas. If he does not do so, it is not because he knows a better way of organizing them, but because he has not had a sufficient scientific training, and is either incapable or too lazy to organize them at all.

Social factors have contributed in a large measure to the strength of this tradition. Whereas the demand for terminological consistency originated in verbal disputes between scholars, the dominance of deductive systematization is closely connected with traditional methods of teaching. Since teaching became a socially organized function of school masters, every recognized science has been taught by way of communicating to the students a logically coherent set of such available results of previous research as are considered certain. And almost every scientist, when formulating in writing his knowledge of a certain field of reality viewed as a whole, has consciously or unconsciously assumed the rôle of a master teaching his science, or branch of some science, to his readers; between the exposition of a new and original system of science and a textbook the formal difference is often very slight, however widely both may differ from monographic publications.

Now, there is no doubt that the deductive form of exposition is eminently satisfactory for the purposes of authoritative communication of science. It gives the whole body of knowledge communicated an appearance of dogmatic certainty and internal coherence; and since the most general truths formally condition the rest, students who for some reason cannot assimilate all the details and developments of a science may at least learn its "principles," "fundamentals" or "elements," that is,

what is considered its most (if not its only) important part. Moreover, whether intrinsically or simply by force of custom, deductive systematization certainly does seem to use a relatively easy and, so to speak, "natural" way of organizing knowledge: a survey of the total field of established special truths is best achieved if we start with the most general principles, and each new truth is best understood by being connected with other truths already known.

But this is not all. Since for twenty-two centuries logic was essentially deductive logic, and even after that theories of induction remained dependent on the latter with regard to the problem of proof, deductive systematization has had not only the force of custom and the motive of pedagogical expediency, but the power of rational justification behind it. When toward the end of the nineteenth century it became clear that scientific investigation does not follow the rules of logic, this discovery was minimized by the distinction between the "psychological" process of thinking and the "logical" order of truths.[1] To the logician it did not

[1] Typically, this distinction is expressed in a recent book as follows: "Psychologically, reasoning is a temporal event in an individual biography. In the logical sense, however, reason is not concerned with the manner in which ideas or propositions actually succeed each other in our consciousness, but with the weight of evidence or proof. Now, . . . it is very seldom, indeed, that in any active inquiry . . . we start from the right premises and go on from them in a definite order to the proper conclusions . . . When we first ask a question, we seldom have an adequate idea of what it is that we have assumed or that conditions our question. It is only after a great deal of intellectual work that we can see what are the proper premises and implications of our position . . . If, then, we distinguish between the premises which logically justify a conclusion and the psychological starting points from which we jump to arrive at them it becomes extremely doubtful whether

matter how in fact we arrived at our conclusions: there was no logical order in the psychological process in any case, even though the psychologist might find some uniformities in it. The important point was that only those conclusions were valid which, once reached, could be deduced from valid premises in accordance with the rules of logic; and these rules remain always the same, even if the premises differ. Deductive systematization was, therefore, considered the only valid kind of systematization.

This assumption would be true only if there were no other but enumerative induction; for, indeed, the latter involves no logical principles which are not included in deductive logic. But, as we shall try to show, the logic —not merely the psychology—of analytic induction is essentially different, though its distinctive characters have attracted very little notice from logicians, owing to the very custom of organizing its results into pseudo-deductive systems for the use of students.

However, the following attempt, as the reader must be warned, will not appear very satisfactory from a logician's point of view; for this is not a general theory of science, but a mere introduction to analytic sociology. Though it is impossible to avoid the most fundamental problems of logic, we can do no more than touch upon

there is any well-defined psychologic difference between the actual processes of reasoning in inductive sciences like experimental medicine and in deductive sciences like geometry or dynamics. Whatever difference there is must be sought elsewhere.

"But if every inductive inference can be put in the form of a syllogism, what can logically differentiate it from other syllogisms? The answer for purposes of scientific method is to be found in the character of the (generally unexpressed) premise of such inductive syllogisms." p. 117, Morris R. Cohen, *Reason and Nature*, Harcourt, Brace & Co., New York.

them briefly and superficially. Furthermore, though I consider the traditional form of scientific systematization very bad and see the possibility of a radically new form, more in accordance with the logic of scientific research, I still keep personally to the old form. *Video meliora proboque, deteriora sequor.* I discovered the new possibilities too late in life: the old ways had already become deeply ingrained in the functional organization of my personal knowledge. True, they might be changed, but at the cost of efforts which can be more productively used elsewhere. The new form of systematization will probably develop gradually; we already find it in many of the best scientific monographs, and it will be applied to systematic works as soon as the traditional method of teaching science undergoes a thorough and well-merited reform.

References

I shun the task of illustrating the criticisms included in this chapter by references to particular authors and works. Attempts to base sociological theories deductively on generalizations drawn from other, particularly biological sciences are so numerous that no particular instances need to be quoted. There has been a strong inclination in Germany to make deductive methods prevail within sociology itself, and its influence can be observed in many prominent works which tend to base sociological theory on a system of "fundamental concepts" preceding actual research; but this is probably only due to the unsettled methodological situation, and the "fundamental concepts" become heuristic concepts as positive investigation advances. There is hardly any necessity to mention particular instances of the use of statistical methods; for discussions of

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these methods, see the bibliography in Lundberg's Social Research; Sections VIII and IX in *Methods in Social Science* (edited by S. Rice), and the numerous contributions in periodicals during the last ten years, particularly *Social Forces* and the *Publications of the American Sociological Society.*

The use of the method of differentiated cases in sociology during the last twenty years has been considerably influenced by the clinical methods of psychiatry, particularly since the spread of psychoanalysis, and also by social case work, the technique of which (including the collection of records) is quite old, reaching back certainly as far as St. Vincent de Paul, but which only recently, and that mostly in America, has reached the stage of abstract discussion and the formulation of general principles. See e.g.

Richmond, M. E., *Social Diagnosis,* New York, 1917.
Chapin, F. S., *Field Work and Social Research,* New York, 1920.
Sheffield, A. E., *The Social Case History,* New York, 1920.
Queen, S. A., *Social Work in the Light of History,* Philadelphia, 1922.
Healy, W., *The Individual Delinquent,* Boston, 1922.
——— "The Contributions of Case Studies to Sociology," *Publications Am. Sociol. Soc.,* 1923.
Social Forces, VI, 4; VII, 4; VIII, 4.

Even Thomas has given way to this influence (See W. I. Thomas and D. S. Thomas, *The Child in America*). The very term "case study," as used in present discussion, suggests specifically comparative reflection about concrete clinical and social work cases rather than more generally a fundamental logical approach to all social reality for the purpose of building sociological theories. One of the con-

sequences of this close connection between sociology and socio-psychological techniques has been the fact that the method of differentiated cases has been applied predominantly to problems where its application is the most difficult, that is, to personality problems.

This whole recent development is rather unfortunate for sociology, however useful for social practice. For the clinical case and the social work case yield material prepared and organized for practical purposes, but entirely unprepared for scientific use. It is the case of a concrete personality (or a family as a grouping of interacting concrete personalities) which raises a problem of practical control. Obviously, to solve this practical problem all the practically relevant components of the personality and factors influencing personal evolution have to be taken into account in their unique biographic combination. Instead of a specific closed system to be compared with other closed systems, we have an individual as a center of convergence of a vast multiplicity of systems, natural and cultural; instead of a definite process within a closed system to be explained by a definite cause, we have processes involving the whole individuality and forcing the student to invoke multiple causative factors. Of course, a "case study" in this sense cannot be a logically sufficient basis for theoretic generalization: its main advantage is that it gives better material and a more reliable starting-point for enumerative induction than mere superficial observation of a multiplicity of cases which are included in advance under a common formal definition. Cf. the analysis of the methodological contribution of Clifford B. Shaw by Stuart A. Rice, *Methods in Social Science*, pp. 549-565.

Nevertheless, where theoretic interests prevail over interests of practical control, as in the works of Shaw, Anderson, Laswell, Sullivan, Mowrer and others, per-

sonality and family studies will probably result in a gradual substitution of a very different category of "cases," systems and processes, circumscribed and defined for purposes of theoretic analysis and generalization, instead of "cases" of a type originally determined by purposes of social improvement. This separation of social theory and social technique may be helped from the other side by the progress in the logical formulation of practical problems as marked, for example, in the paper of E. W. Burgess included in the book *The Workings of the Indeterminate Sentence Law and the Parole System in Illinois*, 1928.

Among other recent studies founded on the method of differentiated cases may be mentioned, for example; Park and Miller, *Old World Traits Transplanted*, New York, 1921; Park, R. E., *The Immigrant Press and Its Control*, New York, 1922; Thrasher's *The Gang* (where the use of statistics adds nothing to the theory of gangs, only to the problem of their ecological distribution); Jesse F. Steiner, *The American Community in Action*, New York, 1928; the monographs of E. S. Young, Kimball Young, J. F. Steiner, E. W. Burgess, S. A. Queen, F. M. Thrasher, F. Znaniecki, E. S. Bogardus, H. A. Miller, E. B. Reuter in Young's *Social Attitudes*, New York, 1931.

CHAPTER VI

Analytic Induction in Sociology

1. *Abstraction and Generalization*

While in enumerative induction, as we have seen, a certain logical class is defined, and the problem is to find characters common to and distinctive of the particular objects belonging within this class which were not explicitly or implicitly included in the definition, in analytic induction certain particular objects are determined by intensive study, and the problem is to define the logical classes which they represent. No definition of the class precedes in analytic induction the selection of data to be studied as representatives of this class. The analysis of data is all done before any general formulations; and if well done, there is nothing more of importance to be learned about the class which these data represent by any subsequent investigation of more data of the same class. A zoologist who, after a prolonged study of unfamiliar specimens, has described a new species; a physicist who, after a few experiments thoroughly analyzed, has formulated a new law, does not usually expect any more knowledge significant for his problem to be derived from the analysis of other specimens or experiments. If he should, it would mean that his analysis of the given instances has been for some reason inadequate, insufficiently thorough to substantiate his hypothesis, or else his conclusions were

logically faulty; and in such a case, a better analysis and more consistent reasoning might lead to the rejection of the hypothesis that a species or a law as defined actually exists.

Of course, the inductive scientist continually goes on investigating objects or processes already defined and classified, even though he does not doubt the validity of his former definition, for there is always something to learn about individual data: concrete reality, as we have said, is an inexhaustible source of new knowledge. But in such a case the new knowledge does not by the investigation of similar concrete instances merely supplement pre-existing knowledge about the class previously defined: it is supposed to be knowledge about some new class somehow related to the class already known. Thus, the zoologist investigates specimens of a species generally known to find out about its varieties, to define subclasses within the given class; or he tries to determine whether this species could have been genetically derived from some other more primitive one; or, leaving the classification of organisms aside, he tests some hypotheses concerning certain tissues or cells. The physicist may repeat an experiment illustrating a known law in changed conditions in order to find out how the unforeseen effect of a familiar cause combines with the effect of some other cause, and therefore what is the relation of the law in question to another law.

It may be said that analytic induction ends where enumerative induction begins; and if well conducted, leaves no real and soluble problems for the latter. With such a radical difference in logical problematization, the logical procedure should naturally differ widely. While both forms of induction tend to reach general and ab-

stract truths concerning particular and concrete data, enumerative induction abstracts by generalizing, whereas analytic induction generalizes by abstracting. The former looks in many cases for characters that are similar and abstracts them conceptually because of their generality, presuming that they must be essential to each particular case; the latter abstracts from the given concrete case characters that are essential to it and generalizes them, presuming that in so far as essential, they must be similar in many cases.

This is why the method of analytic induction has been also called the *type* method or method of *typical cases*. The terms "type" and "typical" are unhappily also used by statisticians, who have appropriated and diverted them from their original significance. For them the typical case is either the *modal* case, a case which is representative of the largest number of cases, shows the most prevalent characteristic in a statistical distribution, or else merely a *sample* case, an instance selected at random from among all the cases belonging to a class circumscribed in advance by a preliminary definition. Whereas a type originally meant a mould, a pattern after which a multiplicity of individual instances were shaped, and thus was akin to the "*eidos*" or "idea" in the Platonic sense, as prefiguring a class of real data. Leaving its older ontological implications aside, logically a typical case meant a case serving as type to a logical class, that is, serving to determine a class, to define it comprehensively; and not merely one helping to characterize a class already defined. Since the statisticians have the other two terms—"mode" and "sample"—we reclaim the term "type" for its original analytic use. When it will be important to avoid misunderstandings, we may use the terms "*eidos*" and

"*eidetic* case" as alternative to "type" and "typical case."

Thus, when a particular concrete case is being analyzed as typical or eidetic, we assume that those traits which are essential to it, which determine what it is, are common to and distinctive of all the cases of a class. The essential traits of a given animal are assumed to be common and distinctive of all the animals of a species; those of a physical process, to all the processes of a kind. There are no formal logical difficulties in the way of such an assumption. We are always free to constitute a logical class composed of all the data and only the data possessing the same essential characters as any given particular datum. But there are obvious methodological problems. What is essential about a particular datum? What characters deserve to be abstracted from its concrete complexity and generalized as common to and distinctive of a whole class?

There would be no answer to these questions possible at all, abstraction and consequent generalization would be entirely arbitrary, if we had to deal with the concreteness of pure empirical reality as given in unprejudiced observation. The existence of relatively closed systems limits this arbitrariness, and makes possible the first and most important step towards analytic induction. Once we have selected a system as object-matter of study, we know that everything that characterizes it, belongs to it, and goes on within it is relatively essential as compared with all the accidental data which, while accompanying its actual existence, are not included or are explicitly excluded from it as irrelevant. While a thermodynamic experiment is going on in a laboratory room, we know which of the objects and processes in this room are relatively essential as belonging to the

experiment, and which accidental in the sense of irrelevant; and in describing it we are sure to omit such contemporary phenomena as the chairs and pictures, or a flirtation between the experimenter and his secretary. When studying a military regiment as a social group, that which concerns its composition and structure must be regarded as relatively essential as against everything the officers and soldiers are thinking, saying, and doing as private persons without reference to regimental affairs.

But this is not enough for the purposes of science. While it permits us to describe a class of processes as typified by the given physical experiment or a class of groups of which the given regiment is the type, it gives us no indication whatsoever as to the logical bonds between this class and other classes of physical or social data. And to determine a class scientifically, it is not enough to characterize the kind of data belonging under it: it is necessary to show how data of this kind are related to data of other kinds. The task of science is not ended with the formation of general concepts of species and laws: these concepts must be logically organized into rational coherent, and comprehensive theories, like the system of organic chemistry or the theory of thermodynamics. The ultimate significance of abstraction and generalization is that they lead to *classification*, that is, to a systematic knowledge of a certain field of reality as a whole.

The supreme importance of classification in science has remained unacknowledged for a long period, again because of the dependence of induction on deductive schematism. Classification under the influence of mediaeval scholastic tradition was made to precede the study of particular objects: you approached reality with

a ready-made classificatory framework into which every given object or process had to fit. This seemed all right as long as the old belief persisted that one could build *a priori* from ontological considerations a classificatory system in which the essence of things was expressed. But when this belief died out, classification came to be treated by logicians as a mere formal instrument of inductive research.

The criticism of enumerative induction, however, by showing the impossibility of obtaining any knowledge valid for all the data of a class defined in advance which is not already involved in this definition, proves that we cannot avoid the responsibility for our classifications by claiming that they are merely instrumental for future study, provisional surveys of the reality to be investigated. Whether we want it or not, every classification is already a theory, and involves theoretic conclusions about reality which are the result of previous study.

Now, the difference in scientific worth between various classifications is not a simple matter of "truth" and "error." Suppose we have two classifications of animals: one by color, the other by anatomic structure as conditioning physiological processes and mutually conditioned by them. The first includes a theory of animals as colored things, the other a theory of animals as organisms. Neither is false, for animals undoubtedly are colored things just as they are organisms; and yet the first is worth very little as compared with the second. The difference obviously lies in the degree of detail and exactness with which they take into consideration the traits of particular animals while describing each class and determining the connections between classes. The former includes only a few characters in each class description and combines in one class objects

in most other respects different, while separating objects in most other respects similar; the latter includes a great many characters in describing each class, and the differences and similarities which it leaves out of consideration in subordinating and coordinating classes are less than those it takes into account.

The consequence of this inequality in detail and exactness is that the classification of animals as colored things leaves room for many other classifications, each taking different characters into consideration, each equally "true" and equally independent; whereas the classification by anatomical structure tends to eliminate all other classifications by trying to account in its own way for the characters on which these are based. We may classify animals by their sizes, by their shapes, by their voices and by their movements; and none of these classifications will bear any reference to the color classification or to one another: whereas the theory of animals as organisms differentiated by their anatomical structure will claim to constitute the only adequate theory of the data in this field, and will try to substantiate this claim by incorporating the facts on which other theories are based, by explaining similarities and differences in color, size, shape and movement in terms of anatomical structure as correlated with physiological functioning. While not an instrument for further research, it includes a challenge and a stimulus to further research, since as long as there are distinctive characters of the animal world as a whole, or of particular classes of animals, not reduced to anatomical structure and the kind of functional organization which is relative to this structure, its claim to unique validity is not substantiated; for this reduction is only possible by analytic investigation. And if, as is more than probable

(as, indeed, it seems already to be a fact), in the course of this investigation new, previously unknown characters are discovered which prove essentially irreducible to the present zoological classification, the latter will have to resign its claim and, while not invalidated as "false," will be rejected as inadequate in favor of a more comprehensive theory.

Whether, now, a classification is merely one of many possible and independent views of the given field, or whether it can be recognized at the given stage of knowledge as being the only adequate theory of this field, depends on its inductive foundations. Thoroughness in analyzing for the description of a class the particular data from which it is derived is the first and obvious condition of its validity. A reservation, though, should be made at this point. Even thoroughness may be exaggerated—and it is, if it goes so far as to make scientific generalization impossible. The progress of science can be only gradual, and the advance in analytic knowledge of particular data must be paralleled by the development of systematic theories of the total domain. At every stage of this progress there is a certain minimum of thoroughness short of which analysis cannot stop without becoming too superficial to be of any value as material for abstraction; but there is also a certain maximum of thoroughness beyond which it should not reach, if it does not want its laborious findings to be wasted for lack of possible means for their theoretic realization. And no hope can be entertained that an analysis will be thorough enough to be final: reality must remain inexhaustible, either because it already is too rich for our thought to master or, more likely, because it ceaselessly grows in wealth.

But it is the second, correlative condition of adequate

classifications with which we are now mainly concerned; and this condition is more difficult to fulfil, for it demands not only conscientiousness, objective insight and persistence in collecting, observing and analyzing data, but power of mental creativeness, intellectual activity capable of building new theories which incorporate the results of analysis. The problem in its widest formulation can be briefly stated here.

Let us assume there is, at a given stage of scientific advance, a large mass of reliable empirical knowledge agglomerated. This knowledge concerns various classes of data, each class described as fully and exactly as possible and desirable at this stage, by way of a thorough analysis of particular data used as typical or eidetic for a comprehensive definition of this class. What is needed is to organize all this knowledge intellectually, to create a theory which will systematize all the class descriptions into a logically coherent, unified body of scientific concepts; if possible, without leaving out of consideration any essential character of any class. It is easy to string on a slender thread a few leading ideas, a series of disconnected descriptions of various kinds of social relationships or social groups, with all their wealth of distinctive characteristics as disclosed by monographic research. Somewhat less easy, but yet not too difficult, is the construction of a nearly perfect system of concepts into which only such class characteristics are incorporated as fit some hypothetical general principles which have been abstracted after a superficial survey of the field. The difficulty is to combine respect for all the facts as disclosed by case analysis with the recognition that the task of scientific work is not achieved until a theory is created bold enough, simple enough, and comprehensive enough to give these

facts their logical meaning as parts of a rational whole.

Obviously, now, this is possible only if among the many characters defining the data of a class, there is a *gradation of importance*, if some are more essential than others in that they determine in a larger measure the nature of these data, and if these more essential characters are also those which the given class shares with a larger variety of other classes. For instance, the possession of vertebrae is a character which determines the nature of the horse in a larger measure than the formation of his feet, the latter in a larger measure than the shape of his tail; and at the same time, the first character is shared by the species horse with all the species of mammals, birds, reptiles and fish: the second only with a relatively small variety of hoof-bearing animals; while the third is an exclusive character of the species. Such a gradation of importance and generality enables the scientist in his theory to approach, if not fully to achieve, the ideal which combines a maximum of empirical detail with a maximum of logical systematization.

Now, in sociology as in other cultural sciences, we are faced just here with a serious difficulty—one so serious that it has not yet been fully overcome. Given empirically a ready-made social datum as a whole, we are unable to determine with certainty by simple experience and observation which of the characters belonging to it are intrinsically more, and which are less important. We say "intrinsically," for of course data may be judged from the point of view of some extrinsic standards of progress, usefulness, morality, or what not; but such judgments have obviously no scientific validity. Take a conjugal relationship, a secret society, or an

army regiment: any and all of its characters can predominate in the experience of several participants at different moments of their participation. There is no connection whatever between the relative importance of various characters as they appear to the participants and their relative generality. Thus, to a married couple characters which conjugal relations possess only among the adherents of their church may seem much more important than other characters which their relation shares with all the conjugal relations in the world, or even all social relations whatever; to the members of a secret society certain traditional rites of initiation may seem more important than some unobtrusive characters which are common to their society and to all organized groups. Indeed, this lack of parallelism between experienced importance and generality goes so far as to have given birth to the theory that the essential in cultural phenomena is precisely the unique, the original and unrepeatable. Thus, a "cultural historical" or individualising method is opposed to the naturalistic or generalising method. Having eliminated this opposition from the very beginning and claimed that the method of sociology, in spite of the difference in object-matter, must be fundamentally similar to that of natural sciences, at this point we seem to be left without any adequate leading principle to substantiate this claim.

What sociologists should do is this: first, discover which characters in a given datum of a certain class are more, and which are less essential; secondly, abstract these characters, and assume hypothetically that the more essential are more general than the less essential, and must be found in a greater variety of classes; thirdly, test this hypothesis by investigating classes in which the former and those in which the latter charac-

ters are found; fourthly, establish a classification, i.e., organize all these classes into a scientific system based on the functions the respective characters play in determining them. This would be a proper analytic induction in full. But sociologists of empirical interests implicitly assume that comparison is not only the test, but the way to classification. What they usually do is to compare various classes of social data—e.g., social groups, social relationships or social processes—and explicitly or implicitly assume that if a character is more general, shared by a larger variety of data, it must, therefore, be more essential, more important in determining the nature of each datum than a character which is less general, found in some only of the data which are characterized by the former

Unfortunately, this assumption does not work, for there are many general characters which are unessential. Thus, the functions of a great many widely different classes of social groups are suspended during the hours of sleep: and yet this suspension of consciousness can hardly be considered an essential character, since neither the composition nor the structure of these groups is affected by it, any more than a poem is affected by not being recited continuously, or an opera by being produced only at long intervals. Other characters may be general and important, but not for the theory of that part of reality with which we are dealing. Thus, every human individual, whatever his social position, has certain properties as a physical body, certain others as a living organism; and yet most of these, however important for physics or biology, are unessential for the sociology of the individual in so far as they play no part in determining his social position and the functions he performs in the community. At best, they belong to

that total set of material conditions within which social life is going on, but which do not explain it any more than, say, the laws of mechanics, while obviously applying to all chemical bodies, explain chemical processes.

As a result of the predominant reliance of empirical sociology on the comparative method and the assumption that what is general is important, we find in practically every field of sociological investigation a number of distinct, independent, non-conflicting classifications. Every school attaches itself to different general aspects of social data, and builds a different theory: and while each of the older authors still boldly claimed unique validity for his particular theory and combatted all other views, present authors have learned that with the methods they are using such claims cannot be substantiated, and thus have come to exercise complete mutual tolerance. Look at the innumerable classifications of social behavior by social psychologists, the classifications of social processes or those of social relations by sociologists. Of course, the significance of this multiplicity of mutually irrelevant classifications is not sufficiently realized, because of the persistence of the old belief that classification is not a final theory, but an instrument, and that consequently no full responsibility need be taken for it; still, such multiplicity would have been impossible, if there had been some reliable standards for judging which characters of social data are more and which are less important.

In short, analytic induction in sociology, just as in the older and more perfect sciences, needs principles of scientific abstraction other than comparison,—principles which will allow it, first, to draw a general hypothesis from a single instance and then to substantiate it by

comparing it with hypotheses derived from other different instances.

Such principles exist and can be fully applied in the social domain. They are: the *principle of structural dependence* and the *principle of causality*. The former leads to *static laws* and to a *genetic classification of social systems;* the latter to *dynamic laws* and to a *functional classification of social changes.*

2. *Structural Dependence and Static Law*

In speaking of characters as essential and as more or less important, we have been using these terms with their current and traditional meanings: an essential character is one which belongs to the essence of the object, and thus determines what this object shall be; and an essential character may be more or less important, depending on the measure in which it determines the object. Now, however, it is time to raise certain problems as to terminology. A "character" can be essential and important only conceptually, for the purposes of the definition of the object; when we say that a character "determines" the object, it is merely an abbreviated way of saying that knowledge of it logically conditions the knowledge of the object, not that the character itself ontologically conditions the object itself.

But after having seen that it is impossible to obtain adequate knowledge of a field of reality under the assumption that logically important characters are those which are general, we are bound to subordinate logical determination to ontological determination, and direct our investigations to the discovery of "something" in the objects which really conditions these objects, and the possession of which by the objects will therefore,

and only therefore, have to be viewed as a character conceptually essential. For example, the possession of vertebrae is regarded as a conceptually essential and very important "character" for the classification of animals, because the zoologists have discovered that the vertebrae really condition in a considerable measure the whole anatomical structure of the real living animal.

What can this "something" be, now, which really conditions social data as objects of sociological research? In looking for an answer to this question, we must separate problems of static and dynamic conditioning, real determination of social systems such as they are, and real determination of processes in which social systems change, become different from what they were. We begin with the first.

As the social system is constructed and maintained by activity, it would seem obvious at first glance that the "something" which determines it ought to be sought in the activity which constructs it. And, indeed, this is what some of the systematizing and classificatory sociological theories have been doing. Active tendencies viewed as psychological realities, by whatever name they are called—instinct, wish, desire, emotion, attitude, sentiment, will—were, and still are, used to define and classify social systems. Here belong, for instance, the various socio-psychological theories of social actions, and the psychoanalytic interpretations and classifications of human relationships and of social personalities. For many years the present author himself used tendencies called by old psychological terms to classify, though not to "explain" social actions, until it dawned on him that the tendency (or "wish," "sentiment," "attitude," or whatever it may be called) is not a component of the system, does not belong to it as, say, the vertebrae be-

long to the animal organism, but is the constructive factor underlying the system as a whole and defined only by its total manifestation. Consequently, classifying social systems by their "subjective" background or source is a mere abbreviation which adds absolutely nothing to the knowledge we already have of them, but only *sums that knowledge up*. When we speak of certain actions as manifestations of the desire for response or the protective sentiment; when we claim that unconscious libido underlies the relation of mother and child, or define personalities as extravert and introvert, it does not mean that we have found there the most important characters or components by which these systems are to be classified, but only that, having already classified them on some grounds, we name each class in terms of a tendency, this being the shortest way to express our concept of this class.

We must, then, look within the social system itself, as an empirical datum, for that which really conditions it in a greater or smaller measure and is consequently more or less important for its scientific description. This means, of course, that the system must be defined neither in terms of its qualities nor in those of the underlying tendencies, but in those of its elements and their relationship, or its real—not its ideal—components. In the same way, the various "characters" on which the anatomical classification of animals are based are distinct elements, "organs," real components of the biological systems classified—unlike the color, the shape or the size, which can be only abstracted but not separated as real objects from the system which they characterize.

However, the assumption that elements really con-

dition a system and that consequently a system can be best described and classified by its analysis into elements has often met with strong opposition, particularly in the sciences of culture. While acknowledging analysis in the idealistic sense, as the isolation by abstraction of simple characters as ideal components of a concrete empirical content, many thinkers reject realistic analysis of a cultural whole into elementary parts, claiming that the whole is more than a combination of parts; that a part separated from a given whole is something entirely different from what it was in the whole, and cannot be treated as an element, since elements by definition remain essentially the same in different combinations; and that consequently, as Comte has already claimed, here the whole explains the parts, not *vice versa*. Thus, the fragments of a painting, the sentences of a poem, the myths or rites of a religion are not elements in the same way as the bodies of a mechanical system or the atoms of a chemical compound; the painting, the poem, the religion must be comprehended as an indivisible organic whole and classified by the characters it shows in its totality.

And yet, the actual progress of scientific research in the most advanced cultural sciences such as linguistics has been toward realistic analysis; nor are the arguments against it justified. For while it is a fact that the elements of a cultural system are somewhat different within this system than elsewhere, it is also a fact that the same or similar elements can be found elsewhere, in other systems. The same words or sentences occur in many different poems; the same aesthetic motives recur in various paintings; the same myths and similar rites are found in different religions. Research in ethnography and folklore continually and successfully utilizes

the concept of elements recurring in various combinations and spreading independently of any particular combination. Furthermore, in characterizing a system by its elements, we do not deny the priority of the whole over its parts: far from it.

Indeed, if the elements in their relationship condition and consequently characterize a cultural system, it is only because these elements have been selected and combined with reference to the system as a whole. The activity which constructs the system chooses every element for the sake of the system which, though not ready until constructed, is in the course of its construction actively anticipated (which does not mean "planned" or "imagined" in advance) by the active tendency. In the principle of achievement we have expressed this anticipatory active determination of the system as a whole. When, now, an element has been chosen and incorporated into the system, it conditions the system in the sense that all the other elements and their relationship have to be adapted to it, if achievement is to be made possible. But this conditioning of the system by the elements is the outcome of the choice and incorporation of the element into the system; the activity constructing the whole has made this part a condition of the whole for the sake of the whole, has given it whatever real importance it possesses.

In so far, thus, as the element fits into the system, as it was made to be for the achievement of the system, the real conditioning of the system by the element does not mean that the element actively influences the system, but simply that its existence within the system is a necessary, though not a sufficient condition of the realization of the latter. We might express this distinction in the formula that *the composition of the*

system is determined by its structure; its existence is conditioned by its composition.

When, therefore, a student of culture describes and classifies a cultural system by analyzing it into elements and investigating the relationship between these elements, his procedure is perfectly justified and does not necessarily imply that he is atomizing or mechanizing cultural reality; provided, of course, the elements and their relationships are arrived at by legitimate methods of experience and observation, and not by arbitrary reduction of the given reality to some other kind of reality, e.g., social relations or groups to individual psycho-biological processes.

There is, though, the old temptation of the analyst to rationalize cultural systems, to treat their intrinsic order, such as it is, as a mere approximation to a more perfect order, perfection being conceived either in terms of teleological or of formal logical consistency. In the social field, the French and English rationalism of the eighteenth century and the first part of the nineteenth century furnish many instances of an interpretation of social systems as organized on the basis of the relation of means to ends. Much more recently, a different type of rationalistic simplification came to be used in sociology, probably originating in abstract legal construction of social situations. Social systems are here explicitly or implicitly assumed to possess an intrinsic order similar to that of deductive logic: given certain fundamental elements and formal principles, the whole composition and structure of the system follows with a kind of logical necessity. Simmel's analyses often approach this type; Eubank has recently done something similar. But the most thorough and systematic development of this method is found in the works of C. Znamierowski, my

colleague at the University of Poznan. I cannot here criticize this approach, for the discussion would involve the whole problem of deductive science. The chief reason why I oppose both types of rationalization is that the intrinsic order which is approximately realized in social systems represents a specific variety of structural organization, with standards of consistency essentially different from both those of teleology and those of deductive logic.

The problem is to discover among the elements of the given social system that gradation of importance which the comparative method is seeking in vain among the qualitative characters of social data. While all its elements really condition the system, make it what it is, there must be some which condition it more than others; that is, some whose realization is more necessary than others, if it is to become this particular kind of a system. Of course, any system would be different from what it is, if it did not contain every one of its elements; but it would be more different if it lacked some particular elements than others. The possession of some of its elements is thus relatively more essential for this system than the possession of others. In so far as this is so, it may be presumed that the class characterized by the possession of those more essential elements includes as a specific variety the class characterized also by the possession of the less important elements, and the latter ought to be subordinated to it, but not *vice versa.*

For example, we find in a certain relation between kindred, say in China, the recognized moral duty of each partner to maintain the other when he cannot support himself, and also the moral duty to assume for the other economic responsibility before outsiders for obli-

gations which he is unable to discharge. Suppose the first element is found to be more essential for this relation than the second, a refusal to furnish the necessary means of subsistence to a kinsman unable to support himself would be a more radical deviation from what their relation was meant to be in the moral intention of the partners and their environment than a refusal to pay his debts to outsiders. It might be presumed, then, that the duty to support a kinsman was, for the purposes of classification, a more general feature of relations between kindred than the duty to take economic responsibility for him before outsiders, in the sense that among kinship relations involving the first duty not all involved the second; whereas all that involved the second must also involve the first. Comparative research would probably confirm this presumption; indeed, it is doubtful whether we can find any socially regulated kinship relation which does not include the recognition of the duty to support a kinsman in need, although there are a number of instances of kinship relations where there is no moral duty recognized to discharge a kinsman's obligations to outsiders.

Since all the elements of a system are interconnected, because selected and organized for the achievement of the system, any gradation of their importance can only mean that some of them in this system depend structurally on others, i.e. that the incorporation of some into the system depends upon the incorporation of others. If in the above example the duty to support a kinsman is really a more essential element of the kinship relation than the duty to discharge his external obligations, this must mean that the latter is structurally dependent on the former, that kinsmen would not consider themselves

economically responsible for each other before outsiders, if they did not believe themselves in duty bound to share with each other their subsistence in time of need. This being so, the presumption that the more essential elements will characterize more general classes of systems than less essential elements can be more exactly formulated as a heuristic principle which we call *the principle of structural dependence.*

This principle presupposes that structural dependence between definite elements is not an accidental feature of an individual system, but belongs necessarily to the whole class of systems characterized by the type of element which has proved dominant in one system. Since structural dependence means that a certain dependent element will not be found in a system which does not already contain a certain dominant element, the principle of structural dependence postulates that a class of systems characterized by the possession of a certain type of dominant element includes all the systems characterized by a certain type of dependent element. But the reverse is not true; for from the fact that a system contains a certain dominant element, it does not follow that it must contain a certain dependent element; and consequently, the principle of structural dependence does not postulate that the class characterized by the possession of a certain type of dependent element includes all the systems possessing a certain type of dominant element, only that it includes some of them. In other words, the class with the type of elements which are dominant is the super-class, the class with the type of elements which are dependent on the former is a sub-class of the super-class; and there may be sub-classes within the same super-class containing different varieties of dependent elements, or a sub-class

where the elements characterising the super-class exist without any elements dependent upon them.

This principle is obviously not applicable to any data taken apart from their system, but to elements included in a system. It is conditioned by the structure and composition of the system as a whole, for the activity which constructs the system not only selects its elements, but also adapts them mutually to one another. Values incorporated into a system become through this mutual adaptation different from the same values as they were outside the system. The identity of each element is not only a matter of the characteristics it possesses as a separate datum of experience, but it is affected by the characteristics it acquires as an element in this very system through its structural connection with other elements, and by virtue of the place which it occupies within the system. Two values with a similar content and meaning taken into two systems are similar as elements of those systems only if the systems are similar, and become differentiated if the systems are different.

Thus, the dependence between two given values *a* and *b* is not absolute: it concerns only their coexistence as elements within a certain system *m* belonging to the class of systems *M*. A seemingly similar value b^1 may be found in a system *n*, belonging to a different class *N*, and not be dependent in that class on any value similar to *a*. This simply means that b^1 within *n* is not the same type of element as *b* within *m*.

Returning to our example: we may find social relations in which each partner is legally responsible for certain economic obligations incurred by the other partner, while there is no recognized moral duty to maintain the other partner in need—as is the case in business partnership. This shows that the structural dependence

of economic responsibility on the duty of support exists only within a certain class of systems exclusive of business partnership. The problem is to determine the class by finding the type of dominant element on which the duty of support itself is structurally dependent. Having begun our analysis with a variety of kinship relations, we shall probably assume that the duty of support depends on the kinship bond, that is, on the supposedly common descent recognized as a basis for social unity. But then we may discover that a similar duty of support in need is found also in conjugal relations, relations of neighborliness, and perhaps in some others, where no common descent is assumed. We must abstract still further and define a type of bond of which all these types are specific varieties. Suppose we find that this common type of bond can be defined as non-utilitarian social unity between the partners of a relation, i.e., the kind of solidarity which exists when two individuals participate disinterestedly (more or less extensively) in each other's personal lives. Business partnership, formed only for the purposes of individual gain under conditions where joint economic responsibility of both partners is necessary to obtain credit, lacks this bond and thus belongs to an entirely different class of relations. Consequently, economic responsibility for one's kin and for one's business partner cannot be duties of the same type: their similarity must be only apparent. Even if their content is similar, their meanings must differ essentially. As a matter of fact, we should probably find on further investigation that their contents are also different. It may prove, of course, that we have made an error in the first place by assuming that there was structural dependence between the given two elements, in which case the whole problem will have to

be restated, but always under the same heuristic principle.

A *static law* is a generalization of facts of structural dependence between specific elements. The scheme of static laws in sociology is: *If a social system belonging to class* M *contains an element of the type* b *dependent*), *it must contain an element of the type* a (*dominant*). Take another instance. Suppose, in studying social groups we find that the institution of ruling authority, i. e., coercive control exercised by representatives of the group and regarded by members as valid, is structurally dependent upon the collective will, i. e., the capacity of the group, recognized by its members, to act as a unit and not as a mere agglomeration of individuals. This permits us to formulate the static law: "If a social group contains an institution of ruling authority, it must contain a collective will."

Obviously, the generality of such a law is derived from the validity of the assumption that *b* is structurally dependent upon *a*. Under the principle of structural dependence, abstraction precedes generalization—as by the rule of analytic induction it should. First, we abstract from a given concrete system certain connections between specific elements, and then we generalize these connections and these elements. Abstraction, indeed, is here something more than ideal separation of characters like color, shape and size from objects which they qualify. As a conceptual isolation of elements discovered by analysis, it is based on the possibility of separating, actually and experimentally, a given element from others. For example, the members of a social group acting collectively may concentrate their attention upon its institution of ruling authority, thus isolating in their experience this institution from the

total composition and structure of the group; more than this, they may modify this institution before changing any other elements (though, of course, not without after-effects). Thus analysis in sociology is more like analysis in chemistry or biology than it is like geometric or philosophic analysis.

What we have called the structure of a system is thus reducible for scientific purposes to a combination of the connections of structural dependence among the elements composing this system. Obviously, this combination has a hierarchical order: the connection between the highest dominant element and the element (or elements) directly dependent upon it conditions the connection between the latter as dominant and the element (or elements) directly dependent upon them, and so on, down to those elements on which no other elements are dependent. Assuming in accordance with the principle of structural dependence that each of these elements as thus connected is typical and each connection expresses a static law, we can by selecting certain connections define the *structural type* of this system and of all the systems of the same class. The definition of the structural type will be most abstract if we take into account only the highest connections in the hierarchy, those of structural dependence between the supreme dominant element and those immediately dependent upon it; it will be least abstract if we include all, down to the lowest connections between those elements which are only dependent and those immediately dominant with regard to them; and the degree of abstractness can vary all the way between these limits. The more abstract the structural type, the wider will be the class of systems belonging to it. Thus, there is a structural type of all social systems in general, structural types of all social

actions, all social relations, all social persons, all social groups, down to structural types represented by the most specialized varieties of these classes.

3. *Ontogenetic Analysis*

If the structural type of a given social system could be determined with perfect certainty, a single case of dependence between two elements of this system would be sufficient to establish a static law, and no more cases would be needed to confirm it. If we were sure that, in a given typical system M, b depends on a, we should expect to find an element similar to a in every system of the same type M where an element similar to b was found; and any exception to the law would be presumed only apparent: either what seemed b could not have been b, or the seemingly absent a must really have been present, and a closer analysis should discover the mistake, or else the system belongs to an entirely different type N, not M. The question is now, can we ever be sure that b depends on a, without falling back in some way on discarded teleology or logic and claiming either that b as means implies a as end, or that b as conclusion implies a as premise?

There is, indeed, a strictly inductive method of discovering structural dependence between the elements of a system: it consists in studying the actual construction of the system, taking the latter (so to speak) *in statu nascendi*. It is evident that, as between a dominant element and a dependent element, the former must be introduced into the system first, for only thus can it really condition the latter in the development of the system, and only thus is it possible for the dominant element to be present without the dependent element.

Structural dependence must be sought, therefore, between those elements which in the course of the system's construction are primarily selected and determined as its components, and those which are added later. Analysis of systems into elements, if it is to lead to static laws, must be *ontogenetic analysis*.

In sciences of nature ontogenetic analysis accompanies, explicitly or implicitly, experimental synthesis: thus, in constructing a physical experiment or a chemical compound, elements are not arbitrarily combined, but a definite order is sought, based on the analysis of previous experiments. The most striking and at the same time most familiar application of ontogenetic analysis is the ontogenetic study of animals: thus, in following the various stages of development of a mammal's fœtus, the structural dependence between particular organs is made most clear. Still, the widest and earliest possibilities of applying ontogenetic analysis should be in the cultural domain, where the spontaneous construction of systems can be most easily observed. Moreover, whatever may be the objections against experimental *modifications* of existing cultural systems, usually nothing but practical difficulties stand in the way of *reconstructing* systems. We are almost perfectly free to write poems, construct scientific theories, establish workshops or commercial enterprises and organize groups for experimental purposes. It is strange, therefore, that in sociology ontogenetic analysis has been so little used, and never with the full realization of its importance.[1]

[1] Some of those sociologists who use logical construction to describe synthetically abstract types of systems are presumably basing their synthesis on previous ontogenetic analysis of individual cases; e.g., Simmel, Vierkandt, Znamierowski, Eubank (Cf. the latter's "Social Processes and the Accompanying Relationships," *Publ. Am. Sociol. Soc.*, 1932).

Historians, naturally, have for a long time been describing social systems in terms of their gradual becoming; but there are several reasons why most of such descriptions can be used by the sociologists only as raw material. First, the historian seldom circumscribes a social system exactly: he is interested in the total set of conditions within which the system exists in time and space, and includes these conditions in his description along with the elements of the system itself. Secondly, he usually does not distinguish methodologically between problems bearing on the construction of a system and those concerning subsequent changes the system undergoes when once constructed. For instance, the formation of a state and its later developments and modifications are for him all parts of the same complex historical becoming. Whereas, as we shall see later on, the sociologist must treat these processes as requiring the application of entirely different methodological presuppositions.

If social systems were perfectly closed and coherent, their construction perfectly unimpeded, and our genetic analysis methodologically irreproachable, a single instance would be sufficient to establish not one static law, but a number of laws. The constituent elements of the system would then be selected and incorporated in the strict order of their importance, and every subsequent element would be structurally dependent directly on the immediately preceding elements and indirectly on the earlier preceding elements.

In fact, however, such an ideal situation is hardly ever found. No social system is perfectly closed and coherent, any more than a biological or a physical system is: for instance, every social group has members who deviate from the recognized pattern; in every com-

plex group organization there are institutions which do not quite harmonize with the others.

Furthermore, in constructing a social system there usually are certain obstacles in some lines, facilities in other lines; and thus the process is hardly ever as orderly as the structure of the system requires. This is particularly striking when overt construction of a social system is preceded by planning. For instance, new groups are often initiated in modern life, and these are usually planned in advance. Once the plan is ready the initiators, foreseeing that certain difficulties will occur at various stages of its realization, begin by removing these difficulties in advance or else provide for their eventual removal. During the realization of the plan other special obstacles may arise, temporarily preventing the full incorporation of certain elements into the system. Thus the normal order of group formation may seem to the observer disturbed or even reversed. It happens, for instance, that the election of officials seems to precede the acceptance of a constitution, though in fact an election implies that a certain constitution has already been agreed upon, at least tacitly, and only waits to be developed in detail and formally accepted. Or a constitution may be formulated by the initiators before the members have even met, and this may be imposed upon the new group, though of course it is not a constitution until it has been accepted by the group.

The existence of such imperfectly systematized or doubtful cases make conclusions drawn from the genetic analysis of any single instance uncertain. And if to these objective disturbances, inherent in the reality investigated, we add subjective sources of error due to insufficient materials, inadequate interpretation or imperfect logic, we must conclude that every assumption

of a structural dependence should always be confirmed by a thorough analysis of several cases.

This does not in any sense mean a return to the comparative method. The individual case is and remains the only source and foundation of a static law. There is nothing positive that comparison as such can teach us about social systems; it is not a basis of generalization. Its only use is negative: it helps us avoid error by giving us several hypothetic generalizations from individual cases to combine. If these hypotheses agree, the law they express is established. If they conflict, more research must be made.

For example, analyzing a particular group—say, a club—in the course of its formation, we find that in this group the institution of ruling authority is dependent on collective will, i.e., that there must be on the part of the individuals forming the group a recognition that the group can act as a whole, and not merely as a sum of persons, before anybody is entrusted with the authority to rule the members in the name of the group and with its sanction. We generalize this structural dependence and claim as a static law, that in every group which has an institution of ruling authority this institution is dependent on collective will. But F. Mirek in a genetic analysis of the Roman-Catholic parish [1] found that the priest, as a person delegated by the Church and institutionally endowed with authority, is the first and dominant element of the parish group, which only begins to exist as such after he has established certain contacts with a number of individuals, obtained response from them, and organized them into a social unit capable of collective action. The two hy-

[1] *The Social Elements of the Roman-Catholic Parish*, Poznan, 1928 (in Polish).

potheses conflict, since the first claims to be a static law applicable to all groups, and the second postulates a different structural connection between the same kinds of elements in a certain class of groups. Either one of the two hypotheses is false, or else the conflict is only apparent; and the problem can be solved only by analyzing more thoroughly and comparing more exactly the instances on which these hypotheses were based, and eventually other similar instances, if there is a possibility that the original materials were incomplete. It will be found after such an intensive investigation that the activity of the priest as organizer of a parish group must be distinguished from his function as ruler of this group after it has been organized. Clubs are often organized also by individuals, and the organizer sometimes becomes president of the club, and yet the two phenomena are different: the first is personal leadership, the second a group institution. Though leadership in certain lines may be institutionalized by groups which have positions of public leadership, yet such positions are not identical with positions of ruling authority. The difference is very clear, if we compare, e.g., the position of president of a golf club with that of the professional golfer in this club. In the case of the priest the two positions coincide in a ready-made parish group, but neither exists as a position within the group before the latter is fully constituted. There is, though, the fact that the priest, while acting as leader during the organization of a parish, has already some kind of official position, whereas the man who organizes a club does it as a private person. The point is that the priest organizes a new parish as a representative of a higher authority, and the parish is meant to be not an independent group free to create whatever institutions it

wishes, but a sub-group of an established church bound to reproduce a pre-existent model of social organization, if it is to belong to this church. Similarly a delegate may be sent to a town by the authorities of a national fraternity, a labor federation, a political party, in order to organize a new chapter, union or branch, bringing with him the model of a constitution already existing, and yet the institutions provided in this model do not begin to exist as institutions of the new group until the latter has convened, commenced to act as a unit, and as a unit accepted the constitution.

Thus, a more thorough analysis of the data confirms our hypothesis that in every group the institution of ruling authority, like all institutions, depends on collective will. The hypothesis of Mirek has to be modified, and then it only supplements ours. The facts which he studied show that there is a certain difference in this respect between a free group and a group subordinated to some other group: the former regulates its collective activities (including the activity of establishing and sanctioning the institution of ruling authority) spontaneously, whereas the latter accepts rules laid down by the group to which it is subjected.

We have given some attention to this example as illustrating not only the character of generalizations reached by genetic analysis, but also the significance of "contradictory instances" in analytic induction. If a datum is merely viewed as a "contradictory instance," i.e., as an individual case in which a hypothesis presumed true proves false, it is scientifically unproductive, for all it does is to impair the logical validity of the hypothesis and force us to substitute a particular for a general judgment. But if we base upon it another general hypothesis, we go beyond mere contradiction, we

have two positive conflicting theories to choose from and the choice can be decided only by introducing new evidence—previously unknown or neglected elements, characters, connections or processes. Thus, further research is made indispensable, and out of it emerge new hypotheses and new problems.

4. Phylogenetic Classification

Structural dependence, as discovered by ontogenetic analysis and generalized in static laws, gives us a firm foundation both for a strictly rational description of what is important in specific classes of social systems, and for a strictly inductive determination of the connection between these classes. A dominant element characterizes a system as belonging to a wider, more comprehensive class than its dependent elements; and within the limits of this wider class there is place for several subclasses, each characterized by a different kind of dependent elements. If the dominant element is A and the dependent element b, the class characterized by the possession of elements A, of which A^1 is a specific instance, may and usually does contain other subclasses in which instead of b, we find elements c, or else d, dependent on A. A is there also diversified, though only secondarily, in the course of the realization of the respective systems. This is a consequence of the dependence of the system upon every one of its elements—here b, c and d. We find, thus, classes $A^1 + b$, $A^2 + c$, $A^3 + d$, which can all be subordinated to our general glass $A + n$ in so far as they all contain essentially similar elements of the type A with some other elements depending on these.

For example, if individual rulership in a group de-

pends structurally on collective will, the possession of collective will characterizes this group as belonging to a more comprehensive class than the possession of individual rulership. Besides groups which possess collective will and individual rulership we may discover other classes. Thus, there are groups in which control over members is exercised by a ruling committee instead of an individual ruler; there are others which possess no ruling authority of any form whatsoever but, relying on voluntary cooperation, have institutionalized leadership. Where collective will is used to support ruling individual authority it acquires a somewhat different character than it has in groups where it manifests itself in following a leader; but these differences are less important for the characterization of these groups than their essential similarity due to their possessing a collective will, i.e. a capacity recognized by members to act as a unit, for without this capacity there would be no group.

The full significance of these principles, however, manifests itself only when, not satisfied with a static classificatory description of social systems, we attempt to explain genetically their variety as a result of progressive differentiation in time. We conceive, then, social reality as having gradually originated in the past and as gradually developing further; not only every particular system, but every known class of social systems came into being some time in the past, and its origin has to be traced to some pre-existing class.

This is the problem of social evolution which in our opinion is exactly parallel, from the methodological point of view, to that of organic evolution. We must only realize clearly that the parallelism lies between organic species as classes of organic systems, animals

and plants on the one hand, and social species as classes of objective social systems (actions, relations, persons, groups) on the other hand. Just as a long time ago there were no horses, potato bugs, pine-trees or roses; and then at a later period specimens of these classes of data somehow came to exist, and still are to be found, more or less numerous: so there was a time when no actions of technical cooperation or written communication, no marriages with educational tasks shared by husband and wife, no relations of employment, no kings, merchants or university professors, no states, churches, army regiments or economic corporations existed anywhere, although later specimens of these classes of data somehow appeared, and still are to be found—no matter whether many or few.

The continued existence of each of these classes of social data, just as of each of these classes of biological data, is assured by the fact that, while particular specimens disappear, other similar specimens appear in their stead. A given class of social systems, like a given class of organic systems, is thus perpetuated by what may be called reproduction: and though the process of reproduction is different, still the very fact of it is sufficient to make us apply the term *social species* to a class of social systems in so far as it is thus maintained in existence, and to speak of *social phylogenesis* as the genesis of new social species.

The task of the sociologist facing the facts of social history stands out clear, though far from easy. Like the biologist, he must combine ontogenetic analysis with phylogenetic research in such a way as to have each of them throw light upon the other and be a test of the other. The result should be an explanatory phylogenetic classification of social systems, at least as cer-

tain as the modern zoological classification—if not more so.

And thus, when the sociologist finds several classes of social systems which, while distinguished by the possession of certain dependent elements (b,c,d), in consequence of parallel static laws are shown to belong to the same general class characterized by dominant elements of the common type A, he will assume that these classes $A^1 + b$, $A^2 + c$, $A^3 + d$ have originated from a genetically prior class by way of differentiation. This prior class must be characterized by elements A, otherwise it could not be the source of the given classes, but not by b, c, or d, or any other element dependent on A, since these dependent elements characterize only the differentiated subclasses. Elements A, as distinctive of systems of this earlier class, must be present, yet entirely undifferentiated, i.e. A^0, not A^1, nor A^2, nor A^3, since their differentiation is only a secondary result of the differentiation of the respective systems after the addition of b, c, or d.

Methodologically speaking, this genetically prior class, characterized by undifferentiated elements A^0 and possessing no b, c, or d, is of course not to be identified with the general class $A + n$, characterized by some later variety of A, like A^1, A^2, A^3, with the addition of b, c, or d. It is a minor matter, though, whether we include formally A^0 under the class $A + n$, assuming $n = 0$, or exclude it altogether. The important point is that, while class $A + n$ only ideally includes the classes $A^1 + b$, $A^2 + c$, . . . class A^0 really conditions them; they could not exist at all if A^0 did not exist. Between the class A^0 and the classes $A^1 + b$, $A^2 + c$, $A^3 + d$, etc. there is a relation of *phylogenetic dependence* corresponding to the relation of *structural*

dependence between an element of the type *A* and an element of the type *b*, *c*, or *d* in the ontogenesis of each particular system of the class $A + n$.

For example, suppose we have found three otherwise similar groups—say, religious societies—one with the individual rulership of a priest, another with the rule of a committee of elders, a third with non-authoritative but recognized leadership of a prophet: these elements all presuppose collective will, that is, they are institutions which could not exist without the support of at least potential group action, as distinguished by group members themselves from their own private activities. The collective will is secondarily somewhat different in each case, owing to the reciprocal influence of the institution dependent upon it: a group subjected to individual rule throws upon its ruler all responsibility for its public actions, but also tends to control every step he makes; one ruled by a committee does less controlling, but also puts less responsibility on the committee; one with institutional leadership gives the leader the least responsibility and control.

Now, we presume that to these connections of structural dependence between elements must correspond connections of phylogenetic dependence between systems. There must be, or must have been, groups otherwise similar which have a collective will, but no individual rule, no committee rule, no recognized leadership, nor any institution parallel to these; and if such groups are found, their species will have to be considered as genetically prior to those other three species, since collective will conditions those institutions, and not *vice versa*.

And, indeed, there seems to be no doubt that religious groupings without rule or institutionalized

leadership do exist and that, while some of them are merely loose collectivities of people interested in religion, others have a stable structure, regular gatherings, and a clear distinction between public and private activities,—between things the group does as a group and things its members do as individuals.[1] Even if one individual plays a more important rôle in these public activities than others, this does not affect the structure of the group, if he does not become publicly recognized as ruler or leader. Thus, we have here the simpler type A^0; and our phylogenetic hypothesis that those other, more complex types of religious groups $A^1 + b$, $A^2 + c$, $A^3 + d$ have originated from A^0, will be confirmed if we succeed in tracing in particular cases the processes of this origin, and show how under certain circumstances a group $A^1 + b$, under others $A^2 + c$, under still others $A^3 + d$ appeared by way of differentiation of A^0.

However, certain important reservations must be made. In particular, we should beware of the well-known deterministic error made by the evolutionary school. Just as in the construction of a system the dominant element does not determine necessarily the dependent element, but only makes it possible as one of several eventualities, so in phylogenetic evolution the earlier type of system does not determine what new systems will evolve, but simply conditions several different possibilities. There is no necessary order in phylogenesis; there are only possible or impossible orders. If systems of a certain kind are given to us, all we know is that their existence opens the way for developments that would be impossible without them.

[1] This seems to be the case in the Molokan group described by Pauline V. Young.

For example in all early social groups we find that a new candidate is introduced into the group by an older member who in a sense vouches for him and at the same time prepares him for membership. Out of this relation between the vouching member and the candidate all the various forms of the educational relation have developed. The consciousness of responsibility of the old member for the candidate and the desire of the candidate to answer to the expectation cooperate in the common intention to have the candidate prove such as a member is expected to be; and from this common intention attempts may arise to have him prepared, if he is not yet prepared. This is the only source from which the educational relation with its mutual duties and obligations can develop. Such a relation is not inherent in the rearing of the offspring by the parent; but if the parents are made to vouch socially for their children, rearing as such is subordinated to social norms and becomes education in the cultural sense. On the other hand, the group may institutionalize the vouching relation by selecting its candidates in advance and entrusting their preparation to somebody who then becomes a public official. We have thus, the two main classes of the educational relation, i.e. between parent and child, and between teacher and pupil, developing out of the same kind of vouching relation, though neither was predetermined by this relation.

Besides this process of evolution by differentiation in which, by the addition of new elements, new subclasses of systems are formed, the opposite process also occurs, which has been called "involution." Two kinds of system which are distinguished by certain elements may become assimilated and at the same time revert to a simpler type by losing these elements. For instance,

various telic groups differing widely in their collective aims may, by gradually dropping these aims through loss of leadership, turn into mere congenial groups ("clubs"), essentially similar in composition and structure.

These remarks on social genesis refer obviously only to closed social systems. There are also genetic processes in which a social system becomes subdivided into several, or several social systems merge into one. For instance, a group may become divided into several groups, or several groups may be united into one complex group. Such processes are only important from the point of view of phylogenetic classification, if their result is a new type of system—if, e.g., in consequence of the division or unification of groups a new kind of group is constructed. What matters then is not how this new kind of group is related to the group or groups whose division or unification gave the impulse to its construction, but what kind of innovation its construction brings as compared with other existing groups of the same general class. For instance, in the formation of the United States of America what matters from the point of view of genetic classification is not the mere process of partial unification of thirteen separate states into one state, but the construction of a new type of federal state.

In general, sharp distinctions must be drawn between the phylogenetic and the historical points of view as well as between the phylogenetic and the nomothetic points of view. For phylogenetic classification the series of historical events as projected on a uniform frame of astronomical time and geographical space is of no importance, except as it imposes on evolutionary possibilities two obvious limits. The first limit is that a

definite species of social systems cannot appear in human history for the first time before the species of systems from which it is genetically derived. The second limit is imposed by the necessity of tradition or, more generally, of some kind of communication as prerequisite of evolution: a new species of social systems cannot evolve out of a pre-existing species except by the agency of people who have somehow, directly or indirectly, experienced systems of the latter species. The alternative would be either some biological continuity of organic human nature which would make every generation behave in similar ways, or else some transcendent duration of "ideas" in a subjective "race memory," or an objective Platonic world, from which the same ideas would emerge over and over again into the field of empirical human action. But since we can no more observe the alleged organic mechanism leading to the formation of, say, a relation of employment or a secret society than discover the process by which supposed race memories sleeping in the limbo of the "unconscious" wake up to activity, the needs of the inductive method force us to appeal to plain empirical facts of historical contact.

But there is no reason whatever for presuming that social evolution must be identical either with the total historical becoming of "mankind" as a whole, or with the sum of histories of particular concrete fragments of "mankind," particularly human "societies" in Comte's sense. Social evolution means strictly and exclusively the totality of those processes in which new kinds of social systems have appeared by way of differentiation (or integration) of pre-existing kinds of social systems, irrespectively of the time or place where such processes have occurred. The genetic bonds between social systems are not historical bonds like those

which unite the successive periods of the total cultural life of a territorial community or a national society; they are bonds of the same type as those which connect a new scientific or philosophic theory with some older theory from which it is derived, a new invention with an older invention on which it is founded, a new style of art with an older style. Evolution in this sense may, so to speak, jump across long periods of time and wide distances in space because, though a particular system is limited in extension and duration, its structural pattern is not; similar systems may be reconstructed any time and anywhere, if the pattern is communicated; and from any one of these a new line of evolution may start.

Several important methodological indications result from this. First, no early type of social systems can be considered definitely "extinct," impossible to reconstruct; for, although in a certain complex civilization conditions might make the reconstruction of certain systems difficult—thus, the deification of the monarch or the relations of slavery could hardly be expected to revive nowadays in the Western world—under other conditions a seemingly extinct type may reappear, either by way of imitative reproduction from the past or by spontaneous evolution of the same kind as its first appearance. Certain classes of social systems of the most primitive kind have, indeed, never ceased to be reconstructed in any civilization: such are, e.g., the elementary social actions of communication, cooperation, avoidance, struggle or protection, and the simple social relations of primary solidarity, regulated sexual intercourse or protection of the immature by the mature.

We mentioned the possibility of the reappearance of a seemingly extinct type of system by spontaneous

evolution, and therein used another important heuristic principle of genetic classification: that evolutionary sequences may be repeated, though of course the repetition is never quite identical. This explains the familiar puzzle of similar systems appearing in different communities with no possibility of direct imitation. There must have been in such cases pre-existing systems of the same type from which the given similar systems evolved, and which have been derived some time from a common source, directly or indirectly. We must assume that, just as the first appearance in the history of humanity of a certain kind of system $A^1 + b$ would have been impossible, if the kind of system A^0 from which $A^1 + b$ is derived had not previously existed, so in the history of any concrete human community Z the appearance of $A^1 + b$ would be impossible, if A^0 has not already existed either in this community or in some other community Y, from which $A^1 + b$ penetrated into Z by imitative reproduction. This principle permits us to test genetic hypotheses, particularly such as refer to systems whose first origin is lost in the prehistoric past. Of course, such testing is only possible if we are perfectly sure that in a given case $A^1 + b$ was indeed evolved from the type A^0 and not simply perpetuated or introduced by imitative reproduction.

On the other hand, however, it is well to remember that from the existence of a genetically primary type A^0 in a given community it does not follow that a particular type $A^1 + b$, originally derived from A, must evolve, for it may happen that instead a new derivative $A^2 + c$ will appear. Suppose we have found that the educational relation has evolved out of the relation between the candidate and the vouching member, it does not follow that whenever the latter relation exists, the

former must appear; for it may be that in a certain society vouching members cease to be interested in the preparation of candidates for independent membership, but instead keep them in subjection and introduce them into their group only as their own dependents, "second-rate" members. This was, e.g., the relation of patrons and clients in antiquity with reference to membership in the city of which the patrons were full citizens.

The difference between a genetic sequence and a causal process can be easily formulated. The genetic sequence is a connection between two numerically distinct systems, one of which has been constructed as a variation of the other; both may even coexist for a while—just as in the organic world. The causal process is a change within one system whose structure is modified as an effect of a modification of its composition. Such a system becomes different from what it was, but preserves its numerical unity. For instance, out of a certain type of marital relations which involve bearing and rearing children, another type may develop genetically to which children are irrelevant: that is, some marriages are concluded which are in certain respects still modelled upon the older type as to the relation between the man and woman, yet are mutually determined to be independent of the eventuality of child-rearing, either by providing against child-birth or by planning to give children when born to the state to be reared. As against this we find cases where the marital relation of a particular couple, originally intended to involve the common duty of rearing children, becomes modified in consequence of the realization that children cannot be born to them or because of the death of their children. Such a changed relation may initiate the pattern of a new variety: indeed, it is probable that intentionally

childless, "companionate" marriages have been derived from marriages unintentionally childless: still, the evolution is not achieved until a number of new relations of the new type are constructed intentionally and independently of the old type.

The causal change of a system into one similar to a type elsewhere existing independently is, we believe, the source of all the phenomena of apparently "convergent evolution," that is, of cases in which a system approaching a certain type B appears, although no system of type A pre-existed, even if B has usually been found to be derived from A. Thus, suppose we find that telic groups, i.e., groups organized especially for the achievement of a certain purpose, evolve from institutional groups, i.e., groups of people performing together a certain function in a wider society. It may happen, though, that a group originally belonging to a very different type, e.g., a territorial group, passes into a telic type by a series of causally determined changes: this is, indeed, what is now happening to modern municipalities, most of which are becoming telic groups for the technical betterment of the material conditions surrounding the city population in space. Another instance: though the profession of physician originates by differentiation from that of shaman, there are instances of shepherds and cowboys becoming physicians in consequence of influences which made them pass from curing animals to curing people; other cases show a different process, as when disabled warriors pass through surgery to medicine; and so on. In such instances, the causal change brings identification with a type which has already been evolved by the normal genetic process.

In short, the evolution of social systems is creative; it goes on by *social inventions*, spontaneous activities

constructing new types of systems as variations of pre-existing types. Variations may be relatively slight and gradual, in which case it is easy to determine the type of system from which a given system developed. If, however, an invention is highly original, the problem of its filiation is complex, for it may have combined structural relationships found in several different classes of pre-existing systems and by making a new synthesis of new elements modified all of these relationships. Up to now, however, in the social field there has been much less originality than in science or technics, where genetic classification can almost never derive a new theory or invention from any one source, and where several distinct sources have to be taken into account.

5. *Causal Changes of Social Systems*

As against the ontogenetic and phylogenetic studies where the composition and structure of the system are viewed as internally determined by the tendency acting in accordance with the principle of achievement, and antecedents condition consequents without necessarily causing them to occur, causal studies must take into account such processes as are found when unintended external influences affect systems, and from definite antecedents definite consequents necessarily result.

The application of the principle of causality to human life has usually been regarded as conflicting with the ideas of spontaneity, creativeness and originality in human activities. And, indeed, the conflict is indubitable, if we attempt to explain causally the performance of an activity as an event occurring in the course of the personal life of an individual or the collective life of a community. But we have eliminated problems of this

kind as insoluble; we do not ask whether the occurrence of a certain activity at a certain time and place is causally determined or free, and if determined then how, because even if we did answer (which we cannot do), this would add nothing to our knowledge of the activity itself, which is entirely defined by the system it constructs, no matter "why" it constructs it. It is not that we ignore "stimuli," but that we find the influence of stimuli altogether relative to pre-existing spontaneous tendencies.

The term "stimulus" symbolizes two distinct concepts. It has been made to designate two radically different kinds of phenomena: experiences which release tendencies hitherto prevented from being active, and factors which interfere with active tendencies. The first concept of stimulus presupposes what we have called the principle of spontaneity; the second, the principle of achievement.

Stimulation in the first sense is nothing more than the appearance within reach of the agent's activity of some value which has already an axiological significance for him, because some tendency of his has selected and incorporated it into a system as a necessary element, but which has heretofore been inaccessible. The removal of the obstacle now allows the latent tendency to use this value and thus to construct actively the system it wanted but could not construct before. From the point of view of the system, which becomes actualized owing to the release of the impeded activity, stimulation in this sense is not a causal process at all, for it does not affect either the composition or the structure of the system: the stimulus is such precisely because it brings nothing new into the system; all its influence is derived from the system as it was before.

For example, meeting an enemy by accident does not become a stimulus unless this enemy was and still is a social value, the object of a social tendency; and what this stimulus will mean depends entirely on the kind of action which this tendency originally purposed to achieve. If the tendency was to avoid the enemy, he will be avoided; if the tendency was to fight him, a fight will be started; if the tendency was to reconciliation, the meeting will release this tendency and an action of reconciliation will begin. Moreover, any one of these actions may start spontaneously without the "stimulus" of the accidental meeting: the agent may originally decide to keep away from his enemy by preventing accidental meetings in advance, or else intentionally arrange a meeting for fight or reconciliation.

It is only the second kind of "stimulation," that by a factor interfering with the achievement of a system, which is of importance for the problem of social (or, generally, cultural) causality. However, such a factor, as will be seen, does not really stimulate an activity in the sense of "rousing it," but only causes a change of activities; we prefer, therefore, not to use the term "stimulus" with reference to it, particularly since in connection with the term "response" it has been used to express a certain approach to human actions which we shall have to reject altogether.

The two points which must be kept in mind when analyzing facts of causation in the social world are that nothing happening within a social system calls for causal explanation which does not constitute a change of the system as a whole, and that nothing can change the system as a whole which does not irremediably conflict with the original significance of its values. Take a system like a parent-child relation or a political party

group. There are all the time events occurring in the intercourse between parent and child, between individual members of the party group, or between the party and other groups, which a student describing the relation or the group need not explain causally. Either such occurrences are originally implied in the very structure and composition of the system, and are an integral part of its existence, or, though unexpected and undesired—as an act of disobedience on the part of the child or a quarrel between group members—they are "counteracted" in time to prevent any consequences for the system as a whole. In other words, such events can be dealt with by the original activities of the systems, the problems they raise are either defined in accordance with its tendencies or can be redefined by the latter in a way which makes them soluble, without disturbing the system.

Furthermore, there may be problems which cannot be actively, instrumentally solved, but are accepted as impracticable and defined as such by the tendencies of the system. However, they raise no doubts as to what the solution would be, if the instruments were available. The tendencies cannot pass into action because of certain obstacles; they remain or become latent as attitudes. But the existence of these obstacles, though it hinders the realization of the system or of certain parts of it, does not modify its structure, does not affect the axiological connection between its values. For example, the parent may wish to give the child certain pleasures or educational advantages, but be unable to afford it, because he is too poor; or, after having given them to the child during a period of prosperity, may become unable to do so during a depression. The members of a group may be hindered from meeting

as often as is needed for the normal functioning of group institutions by difficulties of communication or by political repression of a hostile government. As long as the poverty of the parent does not make the child disrespectful or indignant, as long as the group institutions preserve their positive significance for its members who only wait for more propitious circumstances to actualize them fully, such obstacles do not change the original relation or the original group any more than, for example, the burning of all available copies of a scientific book changes the theory contained in the book, or the impossibility of producing a play because of lack of a theater modifies the content and structure of the play. Obstacles of this kind may be called *technical* in contrast to *axiological* impediments. If we want to explain their appearance, it is not because we must do so in order to understand the system, the actualisation of which they prevent, but because we are interested in them as changes in some other systems: we may want to know the origin of the depression that by striking a family prevents the parents from educating their children, why a certain government pursues a hostile policy toward some party group, why fanatics burn books whose authorship or content they disapprove of.

The changes of systems which demand causal explanation always result from axiological impediments. An axiological impediment is that kind of modification in the composition of a system which makes some of its essential values axiologically conflicting, incompatible with one another from the point of view of the constructive tendencies of this system. For example, when an immigrant child in contact with other children in the community has learned to despise the cultural standards of his parents and to accept instead the

standards of his milieu, which his parents do not share, there arises a conflict of valuation that constitutes an axiological impediment for the continuation of the original parent-child relationship. When the members of a telic group, by participating in a changing civilization, come to regard the purpose of this group as antiquated, the maintenance of this group in its original form meets an axiological impediment.

The difference between a practical, technical obstacle and an axiological impediment is obvious. Popularly speaking, in the first case, the agent wants to do his share in realizing the system as defined, but cannot; in the second he can, but does not want to. It is also clear that, while a technical obstacle may arise within the system itself, if the latter originally is made to include values which cannot be practically realized—as in social utopias—an axiological impediment is always due to factors external to the system, though not necessarily "external" to the personal life of the agents who participate in the system. It is the result of a change which the values of the system undergo when connected with some other values which do not belong to the system.

Now, the effect of such a change in the composition of a system is not, as it would be if there were no spontaneity in human activities, a simple stopping of the activity which the agent does not want to continue, a mere inhibition of the tendency faced by an unexpected axiological conflict among values which must harmonize if the system is to be achieved. The effect is a deflection of the original line of development, a change from the original active tendency to a different active tendency, manifested in an effort to *reorganize* the system, to make it different from what it was. Reorganization

eliminates the conflict of values by substituting within the system a different structure and composition. Sometimes it may consist in breaking the system up into simpler and disconnected systems, each of which will be realized separately: this is intentional dissolution. But never does an axiological impediment interfering with the achievement of a system result in mere "doing nothing," as often happens when a technical obstacle makes its active realization impracticable.

Thus, the parent-and-child relation is not simply dropped when the child begins to despise the parent's standards and apply different standards derived from his social environment: it becomes established on a new basis as a different kind of relation. If no new kind of relation can solve the situation which has developed, it may happen that the child or the parent, or both, will perform definite actions by which the relation will be broken; but in any case something will be done.

The active members of a group do not all of a sudden lose all interest in the group and stop functioning as members when its purpose appears to them worthless in the light of new telic criteria which they have brought to bear upon it from their outside activities. Depending on the composition and structure of the group and the character of their participation, they will reorganize the group by giving it a new purpose, change it from a telic group into a mere companionable group (a "club"), make a part of some other, wider organization, or subdivide it into smaller groups. When none of these solutions appears satisfactory to them, they may decide to dissolve the group, which means that the various elements constituting the group will be broken up and incorporated into other active systems, temporarily or permanently. The only time that nothing will be done

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about it is when the group has already ceased to be actual even before its purpose is condemned, when the members have stopped participating in it and maintaining its institutions; but this means that no development is going on and so no factor is interfering with that development.

After such a reorganization of a system has taken place, the original system can no longer be revived, actualized by a mere removal of obstacles, for the original spontaneous tendency which was manifested in its construction no longer exists. A similar tendency may reappear later on and construct a similar system; but it will not be a return to the status which preceded the change, but a reproduction or a more or less faithful copy of the original system. For instance, some time after a marital relation has been broken by divorce, the couple may remarry; but it will be a new relation, patterned or not on the old, not the old relation continued. When the original purpose of a group has been rejected and its structure modified accordingly, it may happen that later the same members or a new generation of members will again come to believe in the old purpose. Thus the dominant tendencies involved in the collective life of the group will change into a type similar to the earlier one and the group be reconstructed for a second time, in accordance with the original model; but this will be a new causal process, not a reversion to the first causal process.

The question is, how can there be any regularity in the effect, such as is indispensable for the formulation of causal laws. If active tendencies are spontaneous and the effect of the change in composition is the appearance within the system of a different constructive tendency instead of the original one, how do we know

that the tendency which will appear in the same kind of system in consequence of the same kind of axiological impediment will always be the same kind of tendency and no other? How do we know that a given system whose achievement has been interfered with by a definite outside factor will be reconstructed in a certain specific way? How can we tell, for example, that, when in a certain type of group the members reject a certain kind of original purpose or accept a certain kind of new purpose, the group will become structurally reorganized according to a certain pattern and not dissolved or incorporated into another group? Will not a parent-child relation subjected to a change of the child's standards change differently in every case, depending on the various combinations of arbitrary and inexplicable individual impulses of parent and child?

There seems to be no foundation for assuming any such regularity; and yet, as a matter of fact, we find that it can be postulated and that the postulate works. John Stuart Mill found no other ultimate justification for the application of the principle of causality to natural phenomena than the assumption of an intrinsic order, an essential uniformity of nature. It is nothing but a postulate, but a workable postulate. One doubt always remains with the student of nature: whether this postulate refers to nature itself or merely to man's theorizing about nature; whether man truly discovers an order which really and objectively exists in nature, or produces an order which as yet had no existence by organizing his experiences of nature in accordance with the principles of his own activity as a builder of rational knowledge. No such doubts need bother the student of culture. For if there is a regularity in the cultural world, it is really and objectively there, precisely be-

cause it has been produced by man in accordance with the principles of his own activity. Whatever the source of the apparent order which rational knowledge finds in nature, the order of culture proceeds from the same source as the order of rational knowledge, for rational knowledge itself is a part of culture. This does not mean that the social order, the aesthetic order, the technical order, or the religious order is the same as the order of knowledge or identical with different kinds of orders. If this were so, our knowledge of culture might have been ready centuries ago, for all we should have needed to do in order to understand social life, aesthetic life, technical life, or religious life would have been to let our theoretic reason reproduce social, aesthetic, technical or religious systems *a priori* according to its own logic. These orders are different from each other and their "logics" differ from the logic of science. And yet because we are social, aesthetic, technical, religious agents as well as theoretic thinkers, we can understand these orders as well as the order of rational knowledge; and because all of them, the latter included, follow the principles of activity, there must be some essential community underlying their differentiation.

Thus, since spontaneous human tendencies manifest themselves not in chaotic impulsive handling of disconnected data, but in the construction of organized systems of values, it is by no means fanciful to assume that, when their achievement is impeded by an insoluble axiological problem, the reorganization by which this impediment is removed does not proceed either in a chaotic or impulsive way, but follows certain definite lines. Likewise, the capacity for order which shows itself in the creation and maintenance of systems persists still when the systems have to be changed, even though

changes are not spontaneously initiated by activity, but forced upon it from the outside. The postulate of a regular sequence between an external axiological interference with a system as cause, and the reorganization of the system as effect, is supported by our belief that, though culture is not in any sense one rational world, there are no objects nor processes in it which are absolutely irrational and cannot be logically understood, if adequate methods are applied to it.

But, of course, the postulate of social (or, generally, cultural) causality is only valid if its validity can be shown by continuous applications. That is, it must enable us to discover causal laws of changes, and laws which will leave no essentially inexplicable exceptions. Now, we know well that there is no causal law in any science which, when applied to particular instances, is not faced with many exceptions. A strict causal law assumes that, always when, and only when, within a closed system A antecedent p occurs, consequent q must appear: if this assumption is valid, it means that p is the cause of q. Now, in observing such causal processes we may find that p occurs without being followed by q, or q occurs without having been preceded by p. This does not mean yet, however, that the law is invalid, and p is not the cause of q; for the fault may lie with our inadequate knowledge of the particular case. First, the system we are observing may not be A but B, in which case the exception simply means that the law is not applicable within systems B, but does not invalidate their applicability within systems A. Thus, the laws of thermodynamics have proved inapplicable to molecular systems, but remain applicable still to those systems of large masses which were their original field of application. Or if in a system A the supposed cause p

is not followed by its regular effect q, this may mean that there is some other cause x present, the effect of which y together with q forms a resultant v. In such a case we must find x, discover from other cases its regular effect y, and investigate how q and y combine into v. Or else, if the supposed effect q is there, but its cause p cannot be found, it may be that a certain observable fact r represents a combination of p with another fact x; in this case x must have its effect y, and if we find this effect in addition to p, we can discover from other cases the nature of x and investigate how x has combined with p into r.

Whenever, thus, an exception can be explained, that is, can be proved only apparent, not real, we gain not only a confirmation of our previous knowledge, but also new knowledge: we discover the limits within which our causal processes occur or find some other causal process, and thus determine the range of validity of our law or validate some other law. As has been said in discussing induction, it is not the exception that matters, but our attitude toward it: if we refuse to submit to it, but go on analyzing our data, it is a factor of scientific discovery, whereas if we passively accept it, it is check on further progress. Even if the exception should prove real and our causal law be invalidated, the investigation made in defense of this law ought to bring us to the point where we can formulate some other law instead.

In the social field where there are innumerable causal hypotheses formulated through centuries of attempts to control social changes practically, testing every such hypothesis by the analysis of exceptions will prove the most fruitful scientific procedure. Unfortunately, this is very seldom done as yet; usually common-sense

causal generalizations are either accepted in spite of unanalyzed exceptions, or rejected because of unanalyzed exceptions. Sometimes exceptions are used to limit, not the qualitative range of cases to which the law is applicable (as in physical or biological research), but the numerical probability that our application of the law to any particular case will be true (as in the statistical calculations of insurance companies).

This reference to the theoretically unjustifiable use of numbers brings us to the problem of their legitimate use in measuring whatever is measurable in social reality.

6. *The Problem of Quantification*

In all sciences which are dealing with reality, quantitative categories have a theoretic significance only if they concern real quantities, objective characteristics of real systems or elements. When we count the stars on the Milky Way, the trees in a forest, the atoms in a molecule, or the electrons in an atom, we assume that the totality of these elements constitutes a system—astronomical, phyto-sociological, chemical, physical—and we want to determine the size and complexity of these systems as compared with other similar systems in view of some connection between their size and complexity and other characters they possess. When we measure the distance between material things as elements of a special system, we want to know the extension and configuration of this system. We measure the mass of bodies as a real characteristic which these bodies possess in various degrees and which conditions their position and function in certain mechanical systems. We measure temperature, light, electricity, to determine comparatively dynamic characteristics of these phe-

nomena within certain energetic systems. Every quantity in the real world is a characteristic of some fragment of reality which can be quantitatively compared with similar characteristics of other fragments of reality.

What are, then, the real quantities, the measurable characteristics of social reality?

The humanistic coefficient of social data offers us the easiest approach to this problem. It is a fact that men actually treat certain characters of social systems and elements as qualitatively similar, but quantitatively variable. For example, social actions appear to them as more or less friendly or hostile, their objects more or less valuable, their methods more or less effective, social relations are empirically estimated as more or less close or distant, their duties as heavier, or lighter; social persons seem more or less intelligent, their positions higher or lower; social groups are viewed as larger or smaller, stronger or weaker, more or less expansive or exclusive, their institutions more or less stable, their organization more or less centralized. The experiences expressed in such judgments can be compared and the judgments tested by confronting different agents with the same data and letting them estimate the gradation of the same characteristic. Within certain limits and under certain conditions such judgments will agree, and marked disagreements can be traced back to definite differences of standpoint that may be eliminated by experimental testing. There are many experiments to prove this: we might mention those concerning social distance made by Bogardus together with his school, or the graded estimation of personal intelligence and character made by educators and psychologists. In the field of nature, quantification likewise began with a harmonization and organization

of individual quantitative experiences as expressed in judgments of gradation.

However, this is only the first step toward quantification. Quantitatively graded characters of social reality as given in the experience of the agents dealing with this reality are not directly measurable, for no units of measurement can be applied to them. We may, for example, more or less exactly range social relations *A*, *B*, *C* on a scale with regard to the degree of social distance between the partners, but it is impossible to say whether and when the difference in distance between *A* and *B* is equal to that between *B* and *C*. We may determine, basing ourselves upon the experiences of the partners, whether a social relation during a certain period has become more or less distant, but not how much more or how much less.

Moreover, in the social field we find that to every quantitatively variable character corresponds an *opposite* character, and that these are not reducible to each other, but independently real, qualitatively different and varying by themselves. Hostility is not merely a low degree of friendliness; in a social relation distance is not lack of closeness, or *vice versa*, but a distant relation differs in quality from a close relation, and both distance and closeness as empirical characters can be graded separately; a "small" group like a family or an exclusive club is not merely a group which has less members than a "large" group like a state or a political party, but a group which is meant to be small by its own structure as against one which is meant to be large.[1] Nor is it possible to grade social data in such a way that, be-

[1] It could be easily shown that even "stupidity" is not merely a low level of "intelligence," but a qualitatively different characteristic.

ginning with zero, two opposite characteristics will grow in opposite directions: there are no social actions which are neither hostile nor friendly, no relations which are neither close nor distant, no groups which are neither large nor small. But there are, for instance, sexual actions in which various degrees of hostility and friendliness combine, relations showing a mixture of closeness and distance (like that between a Southerner and his Negro servant), large groups that are relatively small (like the state of Monaco), and small groups that are relatively large (like a family with twenty children).

In short, quantitative variations as directly experienced in the social field are still essentially variations in degrees of irreducible qualities; and a degree which is quantitatively higher or lower than another is also qualitatively different. Strictly speaking, the latter is also true of many quantitative variations in the domain of nature; thus, various degrees of heat are experienced as not only quantitatively, but also qualitatively different. Quantification of natural phenomena has become possible only because for each non-measurable gradation a measurable parallel or "equivalent" has been found, which allowed us to ignore as irrelevant for certain scientific purposes the variations of quality which in concrete experience are inseparable from quantitative variations. Thus, in measuring temperature the gradual expansion of certain bodies was substituted as a purely quantitative equivalent for the experiences of gradation of heat and cold as empirical characteristics of reality.

Such measurable equivalents of non-measurable empirical gradations are being introduced into sociology. This is where statistics has already proved useful, and

will prove more useful when sociologists, in applying it, become more careful about determining the specific variable empirical characteristics of social reality for which their quantities are the equivalents. In certain cases the equivalent seems obvious, as when the size of a group is measured by counting its members, its material property by adding economic units; the prestige of a leader is indirectly estimated by the relative number of his followers; the comparative importance of news in the eyes of a certain public by the amount of printed space given to different kinds of news by the periodicals serving this public. In other cases, however, the situation is not so simple. What characteristic of what reality is being indirectly measured when we count the number of crimes or extra-conjugal births in a state, or the frequency of church attendance among the members of a church? Is it the stringency of customs and mores, the coherence of a group, the effectiveness of institutions of public control, or the vitality of personal beliefs and ideals? And when we count the number of births among the families of a class through a number of years, and find that this number is decreasing proportionately to the total number of families in this class, what is it we actually measure? If we take this class in the logical sense, as a concept that embraces all the individual families possessing certain characteristics (say, a certain income, occupation, or habitat), the birth rate as determined by the statistical average is not a characteristic of any particular family, and therefore cannot characterize the logical class as such, for the characteristics of a class are common characteristics of all the cases belonging to this class. If the families of this logical class were parts of one social group, it would be different, because a group as a concrete syn-

thetic unity possesses characteristics which its parts do not possess; but studies of birth rates are usually not meant to measure definite characteristics of concrete social groups.

This neglect of the problem of what real empirical variables are being measured when a statistical tabulation is made has cooperated with other factors (discussed above) in hindering the development of statistics as a technique of quantification, and contributed to its continued use as a method of induction, a way of determining the degree to which qualitative judgments about reality that cannot be fully validated approach validity. It is as if a physicist, instead of measuring temperature by the expansion of certain bodies, counted the bodies which expand as against those which do not, in order to find out approximately how true is the statement that bodies expand.

The slowness of sociological quantification is partly explained, no doubt, by the apparent vagueness of those characteristics of social reality which are given in the experience of social agents as quantitatively variable. Characteristics like the friendliness or hostility of an action, the closeness or distance of a social relation, the intelligence or stupidity of a person, the stringency or looseness of mores, the strength or weakness of a group, the conservatism or progressiveness of its members, seem rather elusive and difficult to separate from other characteristics, even after individual judgments grading social data with regard to these characteristics have been coordinated and made to accord by experimental techniques. Nor is it always clear what the scientific purpose is for measuring them at all in terms of any numerical equivalent. Furthermore—and this is the essential difficulty—what is our reason for assuming

that a certain measurable variable is really the equivalent of some non-measurable variable, that what seems to be a functional connection between the two is not accidental? By what right do we assume that the importance of news is functionally correlated with the printing space given to the news in a periodical, or that the growth of extra-conjugal births is an index of the relaxation of sexual mores?

The way to justify such an assumption is to follow the way physical science has taken in its quantification of nature. The two supposedly correlated variables, the measurable and the non-measurable, must be actually dependent on a common factor which conditions both of them. The experiences of varying temperature and the expansion of mercury in a thermometer are correlated because they are both conditioned by that specific form of energy which is called heat. The graded experiences of the varying heaviness of bodies and weight, as measured by scales, are both conditioned by the attraction between the earth and the bodies on its surface, which acts in direct proportion to mass. And so on.

Now, the only factor on which both measurable and non-measurable characteristics of social reality can be regarded as dependent is found in the active tendencies which create and maintain social systems, giving them a definite composition and structure. If there is a parallelism between the variations in importance of news and the variations in printing space allotted to them, it is because the editor of the periodical tends to establish such a parallelism, and his tendency is adapted to the tendency of the public to treat certain news as more important than other news, and to expect that this importance will be emphasized in the paper

by detailed descriptions and big headlines. If the growth of extra conjugal births is a symptom of the relaxation of sexual mores, it is because in the given community tendencies to conform with traditional regulations are ceasing to be active or conflict with some other tendencies.

But the possibility of connecting scientifically empirically graded non-measurable and measurable characteristics with tendencies on which they depend presupposes that tendencies can be regarded also as quantitative variables. In a word, it must be possible to treat social tendencies as *social forces*, manifested simultaneously both in giving social systems those variable, but not measurable, characteristics which are experienced by people dealing with them, and also in producing certain measurable results. For example, if a religious or national group has the tendency to grow at the cost of other groups, we must be able to assume that this tendency shows itself in a specific structural characteristic of "expansiveness"—a characteristic which observers can grade by comparing this group with other groups, or with the same group at various periods of its duration. We must also be able to assume that this tendency produces a numerical increase of membership by proselytism or assimilation, that is, incorporation of individuals from other churches or nationalities.

The idea that human tendencies (under whatever name) are social forces is quite old: Lester Ward made it the leading principle of his sociology. Unfortunately, the full importance of its implications is seldom seen, and the concept is used rather vaguely and indiscriminately. The main error committed here is that social forces are regarded as psychological realities, located within human agents as psychical or organic

entities, and defined with reference to those entities. Thence such scientifically indefensible metaphysical conceptions as that of Ostwald, according to whom all cultural life is a manifestation of a specific mental energy into which the human organism transforms chemical and physical energy derived from his environment, or the more recent attempts to establish energetic equivalents between the various activities of the individual.

In physical sciences, a force is defined not by its source, but entirely and exclusively by its empirical manifestations; and only after forces thus defined have been investigated and measured is their subsumption under the concept of energy practicable and useful. A force manifests itself by overcoming *resistance*, and every resistance is reducible to another force. The exact measurement of forces is possible only in so far as these forces are determinable with reference to closed systems. If the concept of social forces is to be of real use in sociology, the quantitative sociologist must follow in his field the methodological example of physical sciences and similarly investigate and measure social forces, not in terms of their origin, but of their objective interaction as shown with reference to closed systems. This sets a difficult task for positive inductive research, but a task incomparably more promising than arbitrary speculation by materialistic metaphysics.

Social tendencies manifest themselves in constructing and maintaining social systems; and as long as we limit ourselves to the study of their qualitative manifestations, that is, of the internal composition and structure of different varieties of social systems which they construct, and the qualitative changes which the structure undergoes when the composition is qualita-

tively modified, we do not need the concept of force. The tendency can be defined in purely qualitative terms, and even empirical gradations of certain characteristics of social systems can be ignored unless they show themselves as irreducible qualitative variations.

The use of the concept "social force" becomes necessary only when we want to investigate the problem of *dynamic relationships between systems.* If the values from which a social system is constructed were not the objects of other activities, this problem would never arise. But, as a matter of fact, when we begin to act we find that most, if not all, of the values we want to use already belong to other systems and are interconnected in a way which may be entirely different from, and incompatible with, the way we want to connect them. The man who wishes to build up an important social rôle for himself by getting hold of a political office finds that this office is already in the possession of another man, or has been promised or is coveted by another man whom his friends support. A group in the process of formation that wants to attract certain persons as members finds that these persons already belong to another group. The other man tends to keep his office or his expectations of office; the other group tends to preserve its members and have them loyal and active. The new candidate for office, the new group, cannot achieve the results they tend to achieve unless they succeed in repressing or modifying those other, opposing tendencies. This may be expressed by saying that in order to construct a system the tendency has to *overcome the resistance* of other tendencies.[1] And if we formulate such a relationship between tendencies

[1] The latter can be the agent's own tendencies, as when a person in order to fill an office must retire from business.

in these terms, this means that we treat these tendencies as forces. From the point of view of the given system, the tendencies which construct it by overcoming the resistance of other tendencies may be called *constructive forces*, the tendencies which resist the realization of this system—*obstructive forces*.

Let us take now the point of view of the man who tends to keep his office, the group that tends to preserve the loyalty and service of its members. The tendencies of the new candidate, of the new group, appear from this point of view as *destructive forces*, forces which, if left to themselves, would destroy the personal system of values the officeholder has built around his official position and function, and destroy the group by depriving it of active membership. To maintain those systems, such destructive forces must be counteracted. The officeholder becomes active in opposing the new candidate, the old group performs activities aiming to counteract the influence which the new group tries to exercise upon its members. The tendencies by which destructive forces are prevented from exercising their disorganizing influence upon the system can be called *reconstructive forces*.

These concepts are obviously relative to the systems involved; they serve to define the forces as such with reference to a certain system. But they become scientifically important only if there is some way of measuring a constructive force against an obstructive force, a destructive force against a reconstructive force. For example, the expansive tendency of a religious or national group (which as a qualitative tendency shows itself in certain structural characteristics of such groups as distinct from "exclusive" groups tending to prevent the intrusion of new members), can be regarded as a

social force in so far as it manifests itself in taking members away from other religious or national groups. If the resistance of the latter were invariable, variations of the expansive force of our group could be measured by the relative number of new members it acquired. Variations of resistance would be measurable independently, if we took the point of view of the other group, identified its resistance with its reconstructive force, and could show that the latter manifests itself in preventing a larger or smaller number of members reached by the proselytism of the expansive group from passing over to the latter.

The definitions of social forces here formulated have no claim to be final, and the examples given as illustrations are not assumed to be valid. We merely wish to indicate what seems to us the most promising general direction in which the quantification of social reality might proceed. There are many instances in modern sociology showing that this is indeed the direction in which quantitative research is turning, and it would be profitable work to collect and systematize these instances, taking into account, of course, the fact that progress has been hindered by the inadequacy of qualitative analysis, without which social forces cannot be exactly determined for measurement. We shrink, however, from this work for several reasons, the most important of which is that it must lead sooner or later beyond sociology and into a general science of culture.

For social systems are in dynamic interaction not only with other social systems, but with cultural systems of every category: hedonistic, economic, technical, religious, aesthetic, scientific, linguistic. Constructive social forces meet not only social obstruction, but obstruction from various other cultural tendencies; and reconstruc-

tive social forces have to contend with a great variety of destructive influences. "Social dynamics" must become an integral part of general "cultural dynamics," and unfortunately there are few people with sufficient knowledge of all the various branches of culture to embrace such a vast domain: certainly, the present author is not one of them. Probably the best thing that could be done at this stage would be to start building such a general quantitative science of culture by a systematic, planful, lasting cooperation of a number of specialists: sociologists, economists, religionists, ergologists, philologists, students of art, literature and science.[1]

References

The deep revolution which formal logic is undergoing in our generation has not yet brought any benefit to the methodology of the cultural sciences, however great may be the services already rendered by the new logic to mathematics and theoretic physics. None of the new professional logicians has tried to do for cultural sciences anything like what was attempted by logicians of the old school—Mill, Jevons, Sigwart, and especially Wundt (*Logik der Geisteswissenschaften*, Vol. III of his logical work, last ed. Stuttgart, 1921). The contributions of Husserl's school are, of course, important and valuable, but they have not utilized the results of the new developments in symbolic logic. The cultural scientist who is not satisfied with the principles and rules the older logicians left for his use must, therefore, shift for himself the best he can until such times as please the new logicians to show a greater interest in his specific methodological problems than they

[1] This would be an enterprise worthy of the Social Science Research Council.

do at present, and perhaps also, we might say, a better understanding of them. In view of this situation, doubtless, the logical interests of the sociologists themselves are not so deep and wide-spread as they ought to be; see, e.g., on this point Graham, G. M., "The Logics and the Social Sciences," *Social Forces*, VII. Nor can his contributions be as valuable logically as they would be, if he were primarily a logician and only secondarily a student of social reality. Still, there is a steady advance, manifested in synthetic efforts to evaluate logically the past and present, or to direct the future of sociological research, as well as in a rich monographic methodological literature. Among the former, in addition to the books quoted in preceding chapters, particularly House's *The Range of Sociological Theory*, Eubank's *The Concepts of Sociology*, and *Methods in Social Science* edited by Rice, we may mention Odum and Jocher, *An Introduction to Social Research* (New York, 1929), Ogburn and Goldenweiser, *The Social Sciences and Their Interrelations* (New York, 1927), L. D. White (ed.), *The New Social Science* (New York, 1930), C. A. Ellwood, *Methods in Sociology*, Duke University Press, 1933.

Sociologists are increasingly aware of the difficult problems involved in abstraction and generalization, and the inadequacy of the older method of enumerative induction as exemplified in the familiar works of the evolutionary school. But the only solution most of them see is a combination of the case method—in the sense mentioned in the preceding chapter, as a thorough description of concrete individual data—with the statistical method. Lundberg explicitly states "The chief obstacle to the scientific utilization of case records . . . is the difficulty of . . . generalizing them statistically." (op. cit. p. 173.) Cavan, Houser and Stouffer work on a special technique for "The Statis-

tical Treatment of Life-History Material," *Social Forces*, IX, p. 200. Stuart Rice praises Clifford Shaw for effectively supplementing his case analyses by statistical treatment in *Methods in Social Science*, Analysis 40, pp. 549-565. Reversely, the insufficiency of statistics is used as a main argument not for supplanting it, but for supplementing it by the case method: see e.g. Burgess, E. W., "Statistics and Case Studies," *Sociology and Social Research*, XXII, Jocher, K., "The Case Study Method in Social Research," *Social Forces*, VII, p. 2; Queens, S., "Round Table on the Case Study in Sociological Research," *Publ. Amer. Sociol. Soc.* XXII.

Though every sociologist gives a classification of social phenomena, the problem of the logic and methodology of scientific classifications is not much discussed. After Comte, De Greef, *Introduction à la sociologie*, Paris, 1911, was one of the few who saw the great importance of this problem, though he was chiefly concerned with it for the purpose of a classification of social sciences supplementary to Comte's classification of sciences in general. In recent literature, classification has been for the most part viewed as primarily a problem of listing and systematizing the *sociological concepts* in actual use. This is markedly so when, as a starting-point for a survey of the sociological field, those words of common language are listed which have a sociological meaning, as in Waxweiler's *Esquisse d'une sociologie* or, more comprehensively and exactly, in v. Wiese's *Beziehungslehre*. Eubank, who has made the most thorough critical study of the leading concepts used in sociology, attempts with great ability and conscientiousness the difficult and ungrateful task of classifying systematically the most general of those concepts.

But it seems to me that this is not the essential problem. Every systematization of sociological concepts is already

a *theory of social reality*. A systematization of those concepts which are explicitly treated as heuristic categories—like those contained in the present book—is a more abstract and hypothetical theory than a systematization of those concepts which are reached as a result of later inductive research; but this is only a difference of degree. For heuristic categories would be worthless, if they were not already the results of induction; and the concepts which represent a product of scientific work carried on with the help of these categories would be unproductive, if they did not possess a heuristic character as instruments of future work.

I cannot quite share, therefore, the pessimistic spirit of Eubank in Chapter IV of his book, or that of C. S. Bushnell in his article "Scientific Method of Sociology" (*Am. Jour. of Sociol.* XXV), or of Sorokin in *Contemporary Sociological Theories*, 507 ff., when they find that every sociologist uses a different list of concepts. This comes from the tendency of each of the many sociological theories to deal with a somewhat different range or aspect of social phenomena than the others, and shows that none is a complete sociological theory embracing all the classes of social phenomena which have up to now been observed in all their relevant aspects. But this is hardly to be deplored: rather the contrary. For at the present stage of sociological work it would be too much to expect a complete theory of all social phenomena. If the various sociological theories were already logically unified, this would mean that they had attained such a unification by neglecting the variety of facts for the sake of conceptual harmony and simplicity. Their unification is, of course, a most important goal, but it is not to be reached by a single attempt to organize logically their various and partly divergent concepts, only by building gradually, in many successive

efforts, a more comprehensive inductive theory of social reality which would take into account such results as are embodied in their concepts, viewed not as formal categories but as inductive hypotheses. No doubt, a book like Eubank's ought to be very helpful for this future work in that it gives a survey of those existing sociological hypotheses which no future theory can afford to neglect.

The logical problem of sociological classification is thus only secondarily a problem of harmonizing existing classifications; primarily, it is a problem of improving our *methods* of classifying social phenomena so as to build gradually a new, more coherent and comprehensive theory of these phenomena than the theories now in existence. I agree with Read Bain in "The Concept of Complexity of Sociology" (*Social Forces*, VIII, 1-2), that the complexity of social reality is no more than that of other scientific fields; and if it seems worse, it is because of the imperfect ways in which it has been approached. Now, I believe that the main deficiency in the prevailing ways of handling this complexity is that sociologists have concentrated prematurely on the classification of abstract and elementary *relationships* and *processes*, while neglecting the classification of social *objects*, i.e. social systems and their components. Every scientific theory bearing on empirical reality deals both with classes of objects and with classes of relationships and processes; the former are commonly called simply "classes," and their systematization or "classification" constitutes the descriptive part of science; the latter are called "laws" (static or dynamic), and their systematization is the explanatory part of science.

The example of older sciences shows that classification of objects, that is, rational organization of its primary data, is the first and most indispensable task of a science which has a great variety of empirical material to deal

with, and that it can be more or less satisfactorily achieved at a rather early stage of inductive research, as in biology and chemistry. Whereas a classification of relationships and processes abstracted from these primary data, that is, a hierarchical system of laws, is the ultimate ideal which no science has fully reached yet—not even physics, which has limited to a minimum the variety of its primary data. Sociology in trying to systematize its theories on the basis of such concepts as "accommodation," "conflict," "control," "association," "dissociation," "isolation," and so on, without systematically and thoroughly investigating the vast variety of such primary data as social actions, relations, persons or groups, aims to jump at once to a logical level which (even if it proves within its ultimate reach) demands in any case that broad and firm foundation which only a thorough analytic description and classification of social systems can give.

Ontogenetic analysis of the personality long ago penetrated into sociology from psychology—take, e.g., the psychoanalytic studies of great men; and Shaw's *Natural History of a Delinquent Career* (Chicago, 1931) is a particularly good illustration of this method. But an adaptation of historical-genetic methods to an analytic investigation of closed social systems, particularly social groups and relations, has been scarce; indeed, besides Mirek's book, mentioned in this chapter, I do not know any monograph explicitly and consciously applying this method to determine the elements and the structural type of a particular given group for purposes of generalization. An intimation of the need of such a method is found in a paper by C. H. Cooley, "Case Study of Small Institutions as Method of Research," *Publ. Am. Sociol. Soc.*, 1928.

An example of early phylogenetic classification is Spencer's theory of the evolution of institutions. After that we

find it in a number of works: thus, in Giddings' *Principles of Sociology*, Müller-Lyer's *Die Entwicklungsstufen der Menschheit*, Vol. I, Munich, 1908, and Hobhouse's *Morals in Evolution*. Unfortunately, the genetic differentiation of social systems in the history of mankind has been connected either with Comte's and Spencer's idea of evolutionary stages through which concrete "societies" in the traditional sense are supposed to pass, or with the idea derived from Darwinism of a bio-psychological evolution of the species. Evolution is thus conceived as a succession of total concrete cultures or a development of the cultural life of human beings, whereas for a genetic classification of any category of cultural phenomena—types of religion, of science, of art, of literature, of social groups, persons or relations—these phenomena must be taken as objectively existing realities, detached both from the collective background of cultural communities and from the individual background of human psychology and biology. Since this is seldom done in sociology, phylogenetic classification of social systems has been greatly neglected.

Connected with this neglect of the methodological problems concerning the description and classification of social systems is the lack of exactness in the analysis and explanation of social processes. We leave aside the fact that the very existence of a social system is an active process; for it is a process which involves no change of the composition and structure of the system; but, on the contrary, the balance of active forces maintains the system essentially changeless, as in the case of the various public functions in a conservative group undisturbed by external influences. But even if we include under the term "process," as usual, only "changes," there are still four different categories of "processes," each needing a different treatment.

There are ontogenetic processes, i.e., processes of construction of social systems: organizing a group, becoming a certain type of social person, establishing a social relation. These are repeatable, inasmuch as systems of a certain class can be constructed many times and in many places; but not predictable, inasmuch as we cannot tell in advance to what class or to what variety of a class the finished system will belong. Such processes can be, therefore, subjected to *static* laws, but not to causal laws. Sociology has begun to realize this; but instead of dropping the causal approach and using the genetic approach, it preserves the first, but substitutes "causal factors" instead of "causes" in the strictly scientific sense of the term. Thus, it has been found impossible to subject to causal laws the processes by which individuals become criminals; still, methodologists cling to the principle of causality in the form of "multiple causation." Cf. J. Slawson, "Causal Relations in Delinquency Research," *Publ. Am. Sociol. Soc.* 1928, and see further in *Methods in Social Science*, Analysis 36 by F. N. House of the method of Healy and Bronner, Analysis 39 by R. S. Woodworth of the method of Slawson and Burt, Analysis 40 by S. A. Rice of the method of C. R. Shaw. Whereas if the problematization were changed altogether and the sociologist asked, not what are the causes of youthful individuals' becoming criminals, but what are the necessary conditions without which youthful individuals cannot become criminals, a thorough and strict ontogenetic research would lead to laws of structural dependence which would be both more exact scientifically and more useful for practical control than the present generalizations.

The second category of social processes are changes under external influences of systems whose type is already determined. These are indefinitely repeatable from the time systems of the given type begin to exist anywhere

and as long as they may be reproduced anywhere. These processes (which we have discussed above, sec. 5) can be subjected to exact dynamic laws, causal or eventually functional, and predicted with complete certainty, but like chemical or biological processes only conditionally. Cf. the author's *Laws of Social Psychology* (Chicago, 1925); House's *The Range of Social Theory;* Catlin's *The Principles of Politics.* Unfortunately, reliance on statistical correlation instead of analytic induction from differentiated eidetic cases stands usually in the way of the discovery of exact laws, particularly laws of functional dependence. Most sociologists follow here experimental psychologists and associate two problems which from the logical point of view have no connection whatsoever and methodologically interfere with each other: intensive measurement of causally connected individual processes, and statistical distribution of such processes with a class circumscribed in advance. Cf. in *Methods in Social Science* Allport's experimental determination of group influences upon mental activity as analyzed by Thurstone, and Gosnell's experiments in the stimulation of voting, analyzed by Catlin (Analyses 49 and 50).

Social processes of the third category are processes of genesis of new cultural data from pre-existing data. These processes are creative and therefore inexplicable and unpredictable, though (as we have seen) there is a phylogenetic continuity in the total evolution of social systems, and the emergence of systems belonging to class A^1 from systems belonging to a genetically prior class A can be repeated. This is the kind of processes with which Stuart F. Chapin is dealing in his *Cultural Change,* New York, 1928, and W. F. Ogburn in *Social Change,* New York, 1913. There is no possibility of "laws" of the "phylogenetic" process in any cultural domain, though within

this process there are, of course, many ontogenetic processes (processes of construction of particular systems); and these may be subjected to laws of structural dependence. All laws based on the idea of recurrence of genetic processes are logically ontogenetic laws. Most of Chapin's laws are explicitly of this type; so is Ogburn's law of "cultural lag," Kroeber's "cultural cycles," as exemplified by the change of fashion. (Cf. the methodological appreciation by Allport and Hannan of Chapin's and Kroeber's studies, *Methods in Social Science*, An. 22); Wesley C. Mitchell's theory of business cycles (Cf. *Ibidem*, Clark's analysis, 47); the theory of Dorothy Thomas in *The Social Aspects of Business Cycles*, New York, 1927. From the methodological point of view, the main difficulty is the application of this logical type of law to particular ontogenetic processes. Ontogenetic laws are only valid within closed systems; and while a few students, like Chapin, see this point (in whatever terms they may express it), the majority either ignore it altogether, or implicitly assume that a "society" or "community," or at any rate the total economic, or technical, or political, intellectual, or religious life of a society or a community constitutes a closed system within which laws of structural dependence can be discovered.

This brings us to the fourth category of social processes, occurring within definite limits of extension and duration, specifically in the cultural life of a territorially and temporarily circumscribed human collectivity. A typical illustration of studies of such processes is the collective work of the Hoover Committee on *Recent Social Trends in the United States*, New York, 1933. Processes of this kind are exceedingly complex combinations of innumerable processes of the preceding three types: ontogenetic, causally determined, and phylogenetic. Because of their very complexity, no laws or principles whatsoever can be applied

to them. The only possibility is precisely statistical determination of "trends." A "trend" means simply that within the given collectivity during the given period a specific variety of elementary processes, genetic or causal, increases or decreases in frequency. The purpose of determining a trend is essentially practical; it attracts the attention of those interested in controlling the cultural life of the collectivity to the growing or diminishing practical importance of certain processes, desirable or undesirable; and if the genetic conditions or causes of these processes can be determined by analytic induction, by modifying these conditions or causes, the frequency of the processes themselves can be influenced.

Criticisms which deny the possibility of using the principle of causality in sociology in the same way it is used in sciences of nature draw their strength from the many misapplications of this principle by sociologists. See, for example MacIver "Social Causality," *Publ. Am. Sociol. Soc.*, 1932; Sorokin, P. A., "The Principle of Limits Applied to Problems of Causal or Functional Relationships between Societal Variables and of the Direction of Social Processes," *ibidem.* Very instructive is the fact that in Eubank's thorough book on the concepts of sociology, the bibliography on causation contains chapters from only nine books.

There has been considerable interest during the last ten years in the technique of measurement, although the connection usually maintained between the problem of measurement and that of statistical distribution—a connection typically represented in the psychological "measurements of intelligence"—hinders considerably the progress of effective quantification of social reality. Of course, statistical distribution of elementary phenomena within a system may be sometimes itself a means of measuring, not those phenomena indeed, but the extension of the system which

contains them; thus, demographic statistics and statistical distribution of political attitudes can serve to measure certain characters of states as territorial groups. See e.g. R. E. Chaddock, *Principles and Methods of Statistics*, Boston, 1925; S. A. Rice, *Quantitative Methods in Politics*, New York, 1928.

Of the rich literature on measurements, we may quote:

Bowley, A. L., *The Measurement of Social Phenomena*, London, 1915.

Watson, G. B., *The Measurement of Fair-Mindedness*, New York, Teachers College, 1925.

Elmer, M. C., *Technique of Social Surveys*, Los Angeles, 1927.

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Patten, S. N., "The Theory of Social Forces," *Annals Am. Ac. Pol. Soc. Sci.* (Supplement 1896).
Giddings, F. H., "Measurement of Social Forces," *Social Forces*, I.
House, F. N., "The Concept 'Social Forces' in American Literature," *Am. Jour. Soc.*, XXXI.
Park and Burgess, *Introduction to the Science of Sociology*, Chap. VII.
Eubank, E. E., *The Concepts of Sociology*, Chap. X.

Just before sending this book to print, I discovered that J. Maxwell's *Psychologie sociale contemporaine*, Paris, 1911, has a division of social forces somewhat similar to that given in the present chapter: forces of conservation, of destruction, and of reconstruction. Apart from the use of the concept "forces of conservation" (instead of which I prefer, in view of the fact that a social system is continually being constructed, to distinguish constructive and obstructive forces), the main difference is that these distinctions in Maxwell's theory seem to characterize the forces absolutely, whereas I take them to be entirely relative to a particular system. Of course, no valuation whatever is involved in these terms: whether a constructive force which is constructive from the point of view of a given system will appear positively, and a destructive force negatively valuable, or the reverse, depends on whether we evaluate the existence of this system subjectively as desirable or undesirable.

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